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Charles Murchison M.D.
May. 1863.

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ON

MALARIA AND MIASMATA

AND THEIR INFLUENCE IN THE PRODUCTION OF

TYPHUS AND TYPHOID FEVERS, CHOLERA,
AND THE EXANTHEMATA:

FOUNDED ON

The Fothergillian Prize Essay for 1859.

BY

THOMAS HERBERT BARKER, M.D., F.R.S. EDIN.,

FELLOW OF THE ROYAL COLLEGE OF SURGEONS OF ENGLAND,
PRESIDENT OF THE BEDFORD LITERARY AND SCIENTIFIC INSTITUTION,
LATE PRESIDENT OF THE SOUTH MIDLAND BRANCH OF THE BRITISH MEDICAL ASSOCIATION,
ETC., ETC.

"Still as the breeze, but dreadful as the storm ;"

CAMPBELL.

LONDON :

JOHN W. DAVIES, 54 PRINCES STREET, LEICESTER SQUARE.

EDINBURGH : MACLACHLAN AND STEWART. DUBLIN : FANNIN AND CO.

M.DCCC.LXIII.

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BEDFORD :
W. J. ROBINSON, PRINTER,
SILVER STREET.

TO THE

President, Vice-Presidents, Councillors,

AND MEMBERS OF THE

MEDICAL SOCIETY OF LONDON,

THIS ESSAY

IS MOST RESPECTFULLY INSCRIBED BY

THE AUTHOR.

PREFACE.

THE preface to this book may be very briefly written. In the year 1858, the subject named in the first page formed the title of the Essay for the Fothergillian Medal offered by the Medical Society of London. In 1859, the medal was awarded to me as the successful competitor.

I had permission of the Council of the Society to publish the Essay immediately after the award, and I have been repeatedly pressed by enthusiastic friends to bring the work into the light: but a variety of labours incident to active professional life, have prevented me from fulfilling the task until this moment.

I have diverged a little from the original Essay in the present effort. I have made the arrangement of chapters less cumbersome: and I have added here and there the thoughts and results of a more

extended experience. In a word, the whole of the Fothergillian prize Essay is given and a little more in the way both of experiment and illustration.

In commeneing this work, I determined to avoid altogether the occult and mysterious : to confess ignorance where knowledge shed no light, and at the same time to try if I could place a firm foot on any point which promised to afford support. I have never swerved from this intention, nor regretted the resolve.

Confining myself to the grand question :—Are there malarious poisons? I have taken, if the expression is allowable, a liberal view of the meaning of this question, including under ‘Malaria’ ‘bad airs’ of all kinds, whether such airs are rendered impure by organic or inorganic matters. Further, I have selected particular diseases as the points of commencement in the enquiry, and have tried to trace these back to their causes, rather than from their causes. Thus I have gone on the plan of excluding different supposed possible causes of the diseases, in preference to the method of heaping up evidence in favour of special hypothetical malarious causes.

This principle of research is left for explanation in the volume itself, with but one other remark bearing upon it, viz.: that as I had in commencing my work no dogma to serve, so have I now no task more dogmatical than the one of presenting to others the history of my inquiries, and the simple inferences which, according to the best of my judgment, arise out of these.

One pleasing duty now only remains; to return my deep obligation to all those who have so kindly and so liberally aided me in the publication of my researches.

In the first place my especial thanks are due to the Council of the Medical Society of London, for promptly acceding to my request to permit the publication of the prize essay, which, according to the terms of the announcement of their prize subjects, belongs to them.

I am also indebted to my accomplished friend Mr. James Wyatt of this town, for the interesting account of the geology of the neighbourhood of Bedford.

In the body of the work the names of my numerous coadjutors will be found, with acknow-

ledgements of the important information they have supplied. Such acknowledgements, however, while true to the letter, convey but little of the spirit with which the assistance was rendered: nor can I find other expression than to say to my friends in borrowed phraseology,

“Thanks, and always thanks.”

Bedford, March 5, 1863.

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ESSAY ON MALARIA.

CHAPTER I.

ON THE USE OF THE TERMS MALARIA AND MIASMATA.

Malaria and Miasmata: Verbal Distinctions. True definition of the term Malaria. Author's application of the term. Ancient views respecting Malaria. The question of Malaria as considered in modern times.

The terms malaria and miasmata are often used indiscriminately to designate the same agents or causes of disease ; and although practically this combination of terms is of little importance, yet in a strict sense there is a difference between the two. *Malaria*, taken in its widest acceptation and according to its true meaning, is a term of Italian origin, “la malaria;” it signifies “bad air,” and includes any agent which being received into the lungs, excites symptoms of disease in the animal economy. The inhalation of carbonic acid, or any other well known substance may thus, in

this sense, be considered as a process of receiving a malarious agent. And although the term malaria is generally applied to an emanation from marshes or infected places, it will be but to use it in its strict sense to give it the benefit of its full importance.

The term *miasm*, used in its strict sense as a word of Greek derivation, *μιασμα*, ought in its correct application always to be applied adjectively; that is to say, as a term added to some substantive agent having infectious properties. Liebig even makes a difference between miasmatic and contagious influences. "If," he says, "matter undergoing decomposition is the product of a disease, it is called contagion; but if it is a product of decay or putrefaction of animal or vegetable substances, or if it acts by its chemical properties (not by the state in which it is) and therefore enters into combination with parts of the body, or causes their decomposition, it is termed miasm." He further observes that "miasm, properly so called, causes disease without being itself reproduced." *

Granting, for the moment, that the definitions between the terms malaria and miasmata are literally correct, it is nevertheless impossible, in writing a long dissertation in which the use of a definite term

* Liebig's Chemistry, p. 402.

is required throughout, so to apply these divisions practically as to render the argument everywhere definite. I must therefore at the onset make a grand arbitrary rule, and accept only one term throughout this work. I shall on all occasions use the term *Malaria*, not confining myself in its use to any special agent, but taking it in its true and broad sense as signifying "bad air," that is to say, air, or a gas, or a compound of gases, which being absorbed by the lungs, gives rise to certain specific effects or symptoms, which grouped together constitute a disease.

ANCIENT VIEWS RESPECTING MALARIA.

In past ages the effects of malarious poisons were commonly traced to metaphysical or supernatural influences, 'ad iram Deorum immortalium.' Even amongst uncivilized nations of the present day similar superstitions prevail. Chandler, for instance, in his travels in Asia Minor (p. 279), tells us that the Mahometans attribute plague and other pestilences to certain spirits or goblins armed with bows and arrows, and sent by God to punish men for their sins; and that when these spectres inflict wounds of a black color the injuries certainly prove fatal, but not so when the supposed arrows inflict wounds which appear white; many other mystical deductions of the same kind may be found scattered through similar historical records of the current time.

The Greeks found a poetical personification of the terrible effects of marsh miasmata, in the serpent Pytho killed by Appollo, and the Hydra put to death by Hereules.

Varro (*de re rustica* lib. i., cap. xii.), taking a more scientific view, ascribed the causes of the injurious effects of marsh miasmata to the presence in the air of a multitude of imperceptible little insects, which leaving marshy places penetrate into the body through the lungs and thus produce fatal diseases. This opinion, adopted by Columella, Palladius, and Vitruvius amongst the ancients, was accepted in later times by Athanasius Kireher (Professor of Physies at Leipsie in the middle of the 17th century), and also by L'Ange, a celebrated physician of the same age, who was one of the first to adopt and support in Germany the theory of the circulation of the blood. Linnæus himself seems to have lent the authority of his name to this same view.

Paracelsus attributed the origin of malarious diseases mainly to the influence of the stars. Sylvius de la Boë thought that marsh emanations were produced by the action of saline and sulphureous vapours which arise from noxious spots and alter the composition of the atmosphere. This opinion was taken up and supported by the distinguished Bernard Ramazzini.

The partizans of the humoral pathology (first originated by Galen) maintained that diseases arising

in the neighbourhood of marshes and infected places, are produced by the dissolution and putrefaction of the humours of the body, as the result of the heat and moisture present in the spots where the fevers are accustomed to break out.

Frederick Hoffman argued that the vapours which rise from marshes make the air heavy, deprive it of energy and elasticity, and render it useless for the vivification and expansion of the blood and humours.

The opinions of Hippocrates have sometimes been adduced as supporting particular views as to the nature of Miasmata ; but a careful perusal of the works of this great author leads me to conclude that he attributed the evil effects of marshes to the bad drinking water which they supplied, rather than to any specific poison impregnating the air. In fact, we have the authority of his latest and most learned translator and commentator, Dr. Adams, that there is no passage in his works in which the effects of marsh effluvia in engendering fevers is distinctly noticed, although, adds Dr. Adams, if he were ignorant of this fact it was known to Galen and Avicenna.* It may be that Hippocrates was not ignorant of the supposed influence of malaria, but that he did not recognize this hypothesis, and chose from his observation to conclude that the fever poisons were swallowed rather than inhaled.

* The Genuine Works of Hippocrates. Sydenham Society. By F. Adams, LL.D. Vol. i., p. 349.

Such were some of the opinions held in past time in reference to malarious influences ; they were vague in the extreme, nor was it to be expected that any great impulse would be given in this direction, until the time when the composition of the atmosphere and the process of respiration were more fully made out and understood ;—discoveries virtually of modern growth.

THE QUESTION OF MALARIA IN GENERAL, AS REGARDED
IN MODERN TIMES.

The advances of modern knowledge on the respiratory process and on the nature of the atmosphere, while they have tended largely to introduce a correct understanding of the physical and chemical characters of various poisonous gases, have also placed on a more correct basis the requisite methods of enquiry for the determination of those agencies in the production of disease which still remain unknown.

Researches in regard to malaria, have notwithstanding, been conducted even in these days with a vagueness not very commendable. I will sketch out the principal hypotheses of modern times.

In the first place we find that investigators have divided themselves into two great parties.

1. Into those who have endeavoured to isolate gaseous poisons presumed to be present in the atmosphere, and to test the effects of these.

2. Into those, who having no experimental bent, and given up to the sole invention of hypothetical premises, have chosen to accept as a primary fact the belief in malaria ; and next, without further investigation, to build up hypotheses of disease on the supposed properties of the malaria as disease producing agents. A very easy method of study truly, but less profitable than easy.

I cannot, with any show of scientific acumen, or any love for scientific accuracy, allow myself to be led away by the arguments of those who represent the latter class of writers whom I have described. If there are ever in the atmospheric sea in which we are enveloped, agencies by contact with which our bodies are rendered capable of disease, it must follow that these agents are substantive things : they must then, as substantive things, divide themselves into one of two classes, the organic or the inorganic, and in either form they must subject themselves to rigid examination before they can be accepted as causes of disease.

It is to the separation of these poisons and to their identification, that the observers alluded to under my first head direct their efforts, and to their efforts, together with some additional labours of my own, shall I, in the sequel, ask the attention of the reader.

CHAPTER II.

SUMMARY OF VARIOUS AND SPECIAL VIEWS ENTERTAINED
BY MODERN AUTHORITIES RESPECTING MALARIA.

Hypotheses regarding Malaria: Difficulties of classification. Hypothesis that Malarious Poisons are inorganic. Opinions, analyses, and statements of Metcalfe, Rigaud, Tauquelin, Joseph Brown, Thénard, Dupuytren, and Liebig. Differences of argument deducible from the history of opinions.

It is a hopeless task to attempt a rigid classification of the opinions held by modern authorities respecting the question of malaria. The difficulty lies in the circumstance that few men have a clearly defined view on which to take a determined stand. Pre-disposing causes for instance, are admitted as a part of all hypotheses, and sometimes, nay commonly, are so put as to rank, inferentially, as causes, *per se*.

Without attempting a rigid classification of these modern views, I shall nevertheless aim, as nearly as can be, to place in proximity such theories as may bear a general analogical position. For example, I shall consider together the hypotheses which assert that malaria exist as inorganic poisons,—

or as organic poisons,—or as modifications of the common atmospheric air under the influences of meteorological conditions, such as season, climate, barometrical pressure, hygrometrical state, temperature, and elasticity,—or lastly, as modifications dependent on geological peculiarities.

In this section the reader must not expect a complete history of the opinions of all who have written; that would be too prolonged a task. He must be content to receive representative opinions, through the labours and thoughts of the more eminent authorities on the subjects discussed.

Let us first proceed to analyse that hypothesis which classes malarious products amongst poisons of an inorganic or quasi-organic kind.

The arguments in support of this hypothesis are better supported by Samuel Metcalfe than by any other writer with whom I am conversant. His exposition runs as follows. It will be seen that while he suggests that the malarious poisons are inorganic themselves, he traces them to the decomposition of organic matter.

“It has been repeated a thousand times that the nature of malaria is a profound mystery. But if it be the result of vegetable and animal decomposition, it must consist of *some one or more of the combinations of organic matter in the gaseous state*, such

as *carbonic acid, carburetted hydrogen, phosphuretted hydrogen, ammonia, or some other gaseous product capable of being subjected to chemical analysis.* And it will be seen hereafter, that all the mephitic gases diminish the process of respiration, reduce the temperature of the body, and thus tend to produce a chill, by which nearly all diseases are ushered in,—in short that their influence is to vitiate the nutritive properties of the blood and diminish the energies of life.

We are told by medical authors that fever and many other forms of disease are generated by malaria. But they have never yet informed us what malaria is, nor how it produces intermittent fever in one case, remittent fever in another case, typhus fever, yellow fever, plague, cholera, dysentery, and diarrhoea in other cases. This much, however, is certain, that it proceeds from the decomposition of dead organic matter, the principal results of which are carbonic acid and water, with very small proportions of carburetted hydrogen, sulphuretted hydrogen, and phosphuretted hydrogen. But it is still undecided whether malaria depends on a greater abundance than usual of carbonic acid, or of some other gaseous emanation.

That malaria is not carburetted hydrogen, would appear from the fact that in coal mines, where it (carburetted hydrogen) is evolved in large quantities, ague is a very rare disease, but is very prevalent in

low, damp, and foggy situations, especially during autumn, when the debilitating heat of summer is succeeded by cold nights and mornings. From which it might naturally be inferred that vicissitudes of temperature are essential to a malarious constitution of the atmosphere. And notwithstanding the assertion of Dr. Caldwell, that carbonic acid does not exist in unusual quantities in malarious districts, it is certain that of all the morbid exhalations from decaying animal and vegetable matter, it is by far the most abundant; and that when accumulated in large quantities, it is capable of producing the most fatal forms of fever.

I therefore appeal to the candour of all enlightened minds, whether it is not more in the spirit of true science to ascertain the influence of carbonic acid, and of all other gaseous emanations which are known to arise from the decomposition of dead matter, than to seek for the nature of malaria in some mysterious and hypothetical condition of the atmosphere, about which nothing is known?"

Dr. Metcalfe's leaning is evidently to the opinion that *carbonic acid gas* is the principal malarious agent, as it is the chief emanation from decomposing organic matter, and is, as he thinks, a cause sufficient for the production of spreading diseases.*

* Caloric; its mechanical, chemical, and vital agencies in the phenomena of nature. By Samuel L. Metcalfe, M.D.

In the clinical annals of Montpellier (tom. 44, p. 286,) the following observations occur:—*

“Modern chemists have tried to determine whether the analysis of gases collected in marshes, could not throw some light on the causes of those diseases which result from the action of marsh miasmata; but however praiseworthy their efforts may be, they have not yet advanced the solution of the problem. In fact, the presence in the air of marshes of carburetted and phosphuretted hydrogen, does not account for the effects which are observed in consequence of resorting to those damp and marshy places, since those gases, when inhaled in laboratories, either produce no alteration whatever of the health, or induce death with particular phenomena entirely different from those produced by putrid miasmata.

The oxide of azote has been represented as the general cause of all contagious diseases. According to that opinion, living beings alone would be able to produce the infectious principle, which is consequently independent of marshes and the putrefaction of animal substances; an hypothesis which to us seems completely opposed to almost every known fact, and to the testimony of all observers.

On the contrary, we firmly believe that the emanations rising from substances in a state of

* The author's name is not given in the annals.

putrefaction, are the true causes of diseases ending in irritation of the digestive tube, and produced by miasmata. Such miasmata, we believe, may penetrate into the body either through the cutaneous surface, the pulmonary tract, the deglutition of saliva impregnated with the air in which they are suspended, or lastly, through the ingestion of solid or liquid food.

Let us mention the interesting observations of M. Rigaud, on the dews rising from marshes. As the mists generated in low places never rise very high, and people enjoy very good health above that limit, which does not reach an elevation of upwards of 200 or 300 yards, M. Rigaud infers from this fact, and several others, that aqueous vapours are the vehicle of miasmata, and that a careful analysis of the marsh dew must unveil their nature. He carefully collected a large quantity of that liquid, which was submitted to Vauquelin, who gave the following results of his examination:—

1. This water is colourless and clear; when agitated, light flakes are observable in it.

2. It has a slight sulphurous smell, analagous to that of the white of an egg when cooked.

3. Among the various reagents that have been mixed up with that water, the nitrate of silver, of mercury, and of lead, are the only tests which have produced any effect in detecting the presence of a muriate and of an alkali.

4. The residue of that water was of a yellow colour, its weight was scarcely two or three grains ; its taste was rather saltish ; fire blackened it ; it was effervescible with acids.

My experiments, says M. Vauquelin, prove that this water contains :—

1. A portion of animal matter, the largest quantity of which separated under the shape of flakes, whilst the water remained in the bottles.

2. Ammonia, or volatile alkali.

3. Chloride of sodium.

4. Carbonate of soda.”

It will be seen that this author traced all the malarious disease to *the products of putrefying organic matter*.

Dr. Joseph Brown, in his article on malaria and miasma (vol. iii, pp. 60 and 65,) makes the following observations :—

“The chemical and physical properties of malaria are unknown to us ; the experiments which have been hitherto performed to illustrate its nature, or even to discover its presence, having furnished very unsatisfactory results.

The air collected above the marshes of fort Fuentes was found by Gattoni as pure as that at the summit of Mount Leguone, if not more so ; and M. Deseye obtained in the most confined marshes, as on the most exposed hills, 78 parts of azote, 21

of oxygen, and 1 of carbonic acid, from an analysis of the air.

It is true that MM. Thénard and Dupuytren found that the carburetted hydrogen gas disengaged from marshes, left in water through which it was passed a peculiar and very putrescible matter; and M. Julia discovered that dew gathered in the neighbourhood of marshes contains, likewise, a matter capable of fermentation, but there is no evidence that these substances are malaria; nor, were they proved to be so, do we know anything of their chemical properties, except their capacity of undergoing the putrefactive process.

Even the very obvious question, whether malarious poison is always a unit and the same, or a multiplicity of marsh poisons, is one which the present state of our knowledge does not enable us to answer decidedly.

It may be remarked that the diseases which are reputed to originate from malaria pass in the same subject into each other,—intermittents, for instance, into remittents and inversely; and that of a certain number of individuals residing in or merely visiting the same place, at the same time, and consequently exposed to the same morbid cause, some are attacked with one form of fever, others with another.

It is then tolerably manifest that on the questions of nature and simplicity or variety of malaria we possess no satisfactory information whatsoever.

Though marshes, whether salt or fresh, are prolific sources of malaria, especially in a certain stage of the drying process under a hot sun, this poison is the product besides of various sorts of soil, to which the term marsh is by no means applicable.

In the warmer regions of the earth those collections of low and dense brushwood, or of reeds and grass, which are called jungles, are so familiarly known to be productive of malaria, that jungle-fever is as common a name for malarious disease in southern latitudes as marsh fever is in Europe."

This writer also describes lands which are alternately inundated and drained for the cultivation of rice, and are found injurious to the health of the cultivators and the neighbouring inhabitants; wet meadow lands, especially in warm climates; the half wet ditches of fortifications; the mud which is left by the drying of extensive ponds or marshes by the summer heat; lakes, especially if situated in flat countries; the mud left by the retiring tide in seaports and estuaries; the felling of woods, by which process land previously shielded from the sun's rays (and consequently damp) is exposed to their influence; lands which have long lain in pasture, but newly turned up for the purposes of cultivation; these are so many causes, as he thinks, of malaria and its consequences.

The decomposition of vegetable matter in other circumstances than in connection with soil, has

frequently proved a prolific source of malaria. This is often exemplified by the very pernicious effects of steeping flax and hemp. There are many examples of fevers originating from the decomposition of coffee, potatoes, pepper, and other vegetables.

Neglected sewers and drains have proved under a high temperature, to be productive of fever by generating malarious poison.

It will be found that vegetable matter and moisture are present in all the examples, and that animal matter is so occasionally.

With regard to water, it seems ascertained that its presence is necessary, if not at the surface, certainly below it ; but that the quantity in the former situation should not be large. Many circumstances tend to prove that for the production of malaria only a small proportion of water should exist in any situation.

The necessity of the presence of vegetable matter has hitherto constituted an important part of the creed regarding malaria, but this necessity is questioned by high medical authorities.

Malaria is generated in so many instances in which animal matter does not exist, that we must conclude that the presence of such matter is not essential to the formation of the poison ; whether when present it increases the quantity, or modifies the nature of the miasma, does not appear to be ascertained.

Heat is the extrinsic agent most influential in favouring the production of malaria in soils and situations capable of engendering it,—an influence attributable probably to the effect of a high temperature in favouring the chemical action between organized recrements and humidity, and likewise to its accelerating the formation of the aqueous vapour which appears to be the vehicle for the diffusion of the poison.

Its effect is in general more intense in proportion to the proximity to its source. This is probably owing to the more condensed state in which malaria exists near to the spot where it is generated ; and it is remarked that circumstances which favour its condensation add to the intensity of its effects.

It appears to be on this principle that the vicinity of swamps is so much more pernicious in the evening or night than during the day.

The distance to which marshy emanations may extend by gradual diffusion, has been calculated by Monfalcon to be 1400 or 1600 English feet of elevation, and from 600 to 1000 in a horizontal direction.

Intermittent and remittent fevers are not the sole results of malaria. Besides these its more familiar effects, organic affections of the spleen, liver, and mesenteric glands ; similar affections of the stomach and intestines ; dropsy, apoplexy, palsy, and idiocy, are the effects of its long continued application. Cholera, dysentery, and diarrhoea, are by many

writers referred to mere brief agency of a bad air, and there is reason to think occasionally with justice. Intermittent neuralgia, there seems little doubt, is due to malaria; and to this list some are disposed to add rheumatism, but the propriety of such an addition seems doubtful.

It is observed that the natives of marshy districts, who permanently reside in them, lose their whole bodily and mental constitution, contaminated by the poison they inhale.

By the progress of civilization, and consequently of agriculture, the domain of malaria is diminishing throughout the more enlightened portions of the earth.

But it is remarked that malaria has a peculiar attraction for certain surfaces, and that it is not disengaged from those to which it adheres, at least not in a noxious form; in other words they do not constitute fomites.

The attraction of the poison for trees is great; and it has repeatedly been observed that not merely a few individuals, but the population of whole cities, situated in the most swampy districts in the world, have owed their security to a screen of woods interposed between them and the marshes.

The floors of dwellings are supposed to have a similar attractive power over the poison; and hence, in malarious districts, the second stories of houses are found to be more salubrious than the first.

Liebig, in remarking on stagnant water, observes as follows :—

‘The soluble substance which gives to water a brown colour, is a product of the putrefaction of all animal and vegetable matters ; its formation is an evidence that there is not oxygen sufficient to begin, or at least to complete the decay. The brown solutions containing this substance are decolorized in the air by absorbing oxygen, and a black coaly matter precipitates—the substance named *coal of humus*. Now if a soil were impregnated with this matter, the effect on the roots of plants would be the same as that of entirely depriving the soil of oxygen ; plants would be as little able to grow in such ground as they would if hydrated protoxide of iron were mixed with the soil. All plants die in soils and water destitute of oxygen ; absence of air acts exactly in the same manner as an excess of carbonic acid. Stagnant water on a marshy soil excludes air, but a renewal of water has the same effect as a renewal of air, because water contains it in solution. When the water is withdrawn from a marsh, free access is given to the air, and the marsh is changed into a fruitful meadow.

In a soil to which air has no access, or at most but very little, the remains of animals and vegetables do not decay, for they can only do so when freely supplied with oxygen ; but they undergo putrefaction, for the commencement of which air is

present in sufficient quantity. Now putrefaction is known to be a most powerful deoxidising process, the influence of which extends to all surrounding bodies, even to the roots and plants themselves. All substances from which oxygen can be extracted yield it to putrefying bodies; yellow oxide of iron passes into the state of black oxide, and sulphate of iron into sulphuret.'

Liebig further remarks—

"All the observations hitherto made upon gaseous contagious matters prove that they also are substances in a state of decomposition. When vessels filled with ice are placed in air impregnated with gaseous contagious matter, their outer surfaces become covered with water containing a certain quantity of this matter in solution. This water soon becomes turbid, and in common language putrefies, or to describe the change more correctly, the process of decomposition of the dissolved contagious matter is completed in the water.

All gases emitted from putrefying animal and vegetable substances in process of disease, generally possess a peculiar nauseous offensive smell, a circumstance which in most cases proves the presence of a body in a state of decomposition, that is of chemical action. Smell itself may in many cases be considered as a reaction of the nerves of smell, or as a resistance offered by the vital powers to chemical action."

Respecting the inorganic poisons as causes of diseases supposed to arise from malaria, we see from the above the greatest diversity of opinion even on points of primary and fundamental importance. We observe that the idea of an organic origin even of the inorganic poisons is everywhere obvious, but that when the inorganic character of the poison is most strongly insisted on, its composition and nature are left undefined.

Passing from hypotheses which assign to inorganic poisons the power of producing spreading diseases, to the consideration of those hypotheses which suggest the organic nature of all disease-producing poisons, we shall meet again with no slight evidence of vagueness and uncertainty. But we shall, for all that, have the consolation of knowing that in this line of argument the majority of observers is to be found. I will try, in the shortest space, to present in the next chapter every side of the question ; from one or other of its representative exponents.

CHAPTER III.

SUMMARY OF VARIOUS AND SPECIAL VIEWS ENTERTAINED
BY MODERN AUTHORITIES RESPECTING MALARIA,
(CONTINUED.)

Hypothesis that Malarious Poisons are organic. Opinions and statements of Nacquart, Brittain and Swayne, Mason Good, Dundas Thomson, Grove. The principles of the Zymotic theory as propounded by Dr. Farr.

Nacquart expounds the primary and simplest view of the organic theory. He argues that miasmata are not properly gases, but are either fluids in a gaseous or aëriform state or corpuscles spreading in the atmosphere from which they receive, if not different properties, at least important modifications in those properties. We have seen in later times a somewhat similar hypothesis advanced in reference to the presence of fungi in the atmosphere as causes of disease. The hypothesis of Drs. Brittain and Swayne in reference to the cholera fungus will be called to mind, and also the theories of the late Dr. Snow who believed, as is well known, that cholera is mainly conveyed by water, and thought that the cholera cell, which is his term for the cholera poison,

may float a short distance in the atmosphere, may be inhaled, and may thus give rise to the disease.

Dr. Mason Good in his *Study of Medicine*, (vol. i.), in a most elaborate argument on the question of malaria, in which he diseusses the opinions of Lind, Clarke, Belfour, Chisholm, Blane, M'Grigor, Johnson, Berthé, Fellowes, Pym, Frank, Cullen, Jackson, O'Halloran, and other authors, comes in the end to enumerate the nature of poisonous airs, after a manner not very different from that sustained by Naequart. His enumeration is rendered in the following sentences.

“ 1. The decomposition of dead organized matter, under the influence of certain agents, produces a miasm that proves a common cause of fever.

2. The whole of these agents have not yet been explored ; but so far as we are acquainted with them, they seem to be the common auxiliaries of putrefaction, as warmth, moisture, air, and rest or stagnation.

3. The nature of fever depends partly upon the state of the body at the time of the attack ; but, chiefly, upon some modification in the powers or qualities of the febrile miasm, by the varying proportions of these agents, in relation to each other, in different places and seasons. And hence the diversities of quotidians, tertians, and quartans, remittent and continued fevers, sometimes mild and sometimes malignant.

4. The decomposition of the effluvium transmitted from the living human body, produces a miasm similar to that generated by a decomposition of dead organized matter, and hence capable of becoming a cause of fever, under the influence of like agents.

5. Fever thus excited is varied or modified by many of the same incidents that modify the miasmatic principle, when issuing from dead organized matter ; and hence a like diversity of type and vehemence.

6. During the action of the fever thus produced, the effluvium from the living body is loaded with miasm of the same kind, completely elaborated as it passes off, and standing in no need of a decomposition of the effluvium for its formation. Under this form it is commonly known by the name of febrile contagion. In many cases all the secretions are alike contaminated ; and hence febrile miasm of this kind seems sometimes to be absorbed in dissection, by an accidental wound in the hand, and to excite its specific influence on the body of the anatomist.

7. The miasm of human effluvium is chiefly distinguishable from that of dead organized matter by being less volatile, and having a power of more directly exhausting or debilitating the sensorial energy, when once received into the system. Whence the fevers generated in gaols, or other confined or crowded places, contaminate the atmosphere to a less distance than the emanations from

marshes and other swamps ; but act with a greater degree of depression on the living fibre.

8. The more stagnant the atmosphere, the more accumulated the miasmatic corpuscles, from whatever source derived ; and the more accumulated these corpuscles, the more general the disease.

9. The miasmic material becomes dissolved or decomposed in a free influx of atmospheric air ; and the purer the air the more readily the dissolution takes place : whence, *è contrario*, the fouler as well as more stagnant the air, the more readily it spreads its infection.

10. Under particular circumstances, and where the atmosphere is peculiarly loaded with contamination, the miasm that affects man is capable also of affecting other animals.

11. By a long and gradual exposure to the influence of febrile miasm, however produced, the human frame becomes torpid to its action, as it does to the action of other irritants : whence the natives of swampy countries, and prisoners confined in gaols with typhous contamination around them, are affected far less readily than strangers ; and in numerous instances are not affected at all.

12. For the same reason, those who have once suffered from fever of whatever kind hereby produced, are less liable to be influenced a second time ; and, in some instances, seem to obtain a complete emancipation."

Dr. Robert Dundas Thomson in an admirable paper on the examination of atmospheric air during the cholera epidemic, while holding that the cause of intermittent fever, and other diseases of the same class, is due to the nature of the atmosphere in which the human system is immersed, maintains that no gas known chemically is capable of producing a specific malady. He argues consequently for the mechanical diffusion in the atmosphere of a specific germ or molecule, that is to say, he believes in the organic nature of malarious poison. In favour of this view he quotes the fact authenticated by Boussingault, that the inhabitants of South America are enabled, in some localities, to withstand the attacks of endemic diseases by mechanical applications, such as veils placed before the organs of respiration, so as to sift the air from morbid solid particles. Proceeding on this idea, Dr. Thomson subjected to chemical investigation the atmospheric air of an infected district, with the view of condensing any vapour, or of detaining any solid particles which might be disseminated through the air. The results were negative. In 1854, during the prevalence of cholera, Dr. Thomson repeated these experiments in the cholera wards of St. Thomas' Hospital, and the summary of his observations are as follows:—

1. That in the atmosphere of a cholera ward, mechanical matters were diffused throughout the

air derived from the inmates, that sporules of fungi and germs of vibriones, or vibriones themselves were obtained by filtration from the atmosphere,—all of these bodies being adulterations, so to speak, of the pure oxygen and nitrogen which alone constitute the wholesome predominating constituents of the elastic fluids destined for respiration.

2. That from a ward only partially filled with patients affected with cholera, substances were separated which were mechanically dispersed to the very summit of the apartment, mixed with fungi or their sporules, while no vibriones, unless in the form of faint traces, could be detected.

3. That in the atmosphere of an empty ward, communicating however with a ward containing cholera patients, mechanical matters were obtained, and traces of fungi, and perhaps of vibriones.

4. That in the external air adjacent to an hospital, substances mechanically distributed were likewise found, and sporules with fungi were also detected to a considerable extent, but no vibriones.

5. That in the atmosphere of a sewer, bodies were also found in mechanical diffusion, associated with sporules, fungi, and vibriones.

6. That the air contained under the three first conditions possessed an acid reaction, that the external atmosphere likewise indicated a similar chemical condition, and that the sewer atmosphere was alone alkaline.

7. That although animal and vegetable life seems unequivocally to be diffused through cholera atmospheres, it would be premature to infer a connection between the disease and these organisms until comparative trials have been extensively made on other conditions of air ; and that the present researches must be only considered as a single stone placed as a contribution towards the foundation of a larger structure.*

Mr. Grove, in his work on epidemics, enters more fully than most other authors into the affirmation of the question of the origin of epidemics from organic germs. He compares the organic poison to the seed of plants, and argues that the reproductive powers evidenced in many cases, especially in the small-pox virus, is direct proof of the organic character of the poison, since no known simple, chemical, or inorganic principle is capable of such reproduction. He opposes the view that the development of a specific cell-germ in the process of fermentation, is mere coincidence, and assumes for the germ a special place in the fermentation act. The chemical theory of epidemic disease he thinks would best be sustained by a comparison of the artificial formation of the alkaloids. Lastly, he considers that some of these organic poisons may be taken in by the stomach, but that the majority are received by the lungs.

* Appendix to report of the Committee for scientific inquiries in relation to the Cholera-Epidemic of 1854, p. 119.

The views thus advanced in relation to the organic nature of miasmatic poisons, have recently been further and more systematically extended, under the title, "The Zymotic Origin of Disease." Not to spoil by condensation the best account of this hypothesis, I shall give it in full from Dr. Farr's essay on the zymotic theory, in the Registrar General's report on Cholera for 1848—1849.

"Miasm, properly so called, causes disease without being itself reproduced. * * * Carbonic acid and sulphuretted hydrogen, which are frequently evolved from the earth in cellars, mines, wells, sewers, and other places, are amongst the most pernicious miasms."—Liebig. Miasms produce diseases like ague, without being propagated by contagion ; and the poisons—carbonic acid, sulphuretted hydrogen, and other gases which are given off by organic matter in putrefaction, afford an illustration of their action. The miasm which excites intermittent fever may be designated pyretine ; and if it were not probable that modifications of the marsh miasm induce in certain circumstances remittent and yellow fever, specific names should be found for their principles. Rheumatic fever is apparently caused by a miasm ;* its changes of seat

* The exciting cause of intermittent fevers, rheumatism, (and rheumatic) neuralgia, is generally admitted (?) to be malaria ; and if viewed abstractedly, and with reference to their specific nature, it is probable that malaria is the only exciting cause of these diseases.—Prout on Stomach and Urinary Diseases, p. 80.

can scarcely be accounted for on the hypothesis that it is a local inflammation of the fibrous tissue.

Certain matters which have not yet been analysed produce small-pox, glanders, hydrophobia, syphilis, measles, scarlatina, and other diseases ;—and as it was proposed before to give names to the well-defined diseases produced by poisons, so, for the purposes of reasoning, it will be equally useful to name these specific matters or transformations of matter by which diseases are propagated, either by inoculation and contact (contagion), or by inhalation (infection). The following list exhibits the popular and scientific names of diseases in juxtaposition with the proposed names of their excitors ; and it may be assumed hypothetically, that in the blood corresponding bodies exist which are destroyed, and by the transformation of which the excitors are generated and reproduced. The names in the second column terminate in *a*, except a few in *s*. *Lyssa* (from *λυσσα*, rabies) the old Greek term has been restored by Mason Good ; I propose for the sake of uniformity, to call puerperal fever, *metria* ; mumps, *parotia*, reserving *parotitis* for simple inflammation of the parotids ; croup, *tracheia* ; and the disease from puncture in dissection, *necusia*, (*νεκυς*, the dead body).

ZYMOTIC PRINCIPLES.

| | | |
|-----------|----------|-----------|
| Small-pox | variola | varioline |
| Cow-pox | vaccinia | vaccinino |

| | | |
|-------------------------|------------|--------------|
| Glanders | equinia | equinine |
| Hydrophobia | lyssa | lyssine |
| Syphilis | syphilis | syphiline |
| Infection in dissecting | necusia | necusine |
| Erysipelas | erysipelas | erysipeline |
| Puerperal fever | metria | metrine |
| Measles | rubeola | rubeoline |
| Scarlet fever | scarlatina | scarlatinine |
| Whooping cough | pertussis | pertussine |
| Dysentery | dysentery | { enterine |
| Diarrhœa | diarrhœa | |
| Cholera | cholera | |
| Influenza | influenza | influenzine |
| Typhus | typhus | typhine |
| Plague | pestis | pestine |

The existence of gangrenine, ergotine, ophthalmine, tetanine, miliarine, diphtherine, parotine, apthine, tracheine, may also be admitted. It is maintained by some pathologists, that the same specific poison produces several of these diseases, erysipelas, necusia, and metria for instance, but while the diseases are described as distinct, it will be most convenient to consider their excitors as distinct, although they may be convertible into each other, and be as nearly related as varioline and vaccinine.

The chemical composition of these principles is at present unknown ; but as salts are distinguished from each other by their relations to other bodies, and though they may have the same appearance in solution, are found to differ by the compounds which they form with other bodies in solution ; so the existence is demonstrated by the effect of the matter

here called "lyssine" on animals, although it cannot be detected by the rough analysis of artificial chemistry. The smallest quantity imaginable of lyssine inserted under the skin of a dog produces hydrophobia; and the bites of the infected dog will throw other dogs, and even human beings, into a state similar to that of the dog from which the charge of lyssine originally came. Varioline in the same manner produces small pox, if the patient has not previously undergone its influence, or the influence of vaccinine—a modification of varioline. The diseases of this class have been frequently spoken of as fermentations; and Liebig has now opened the way to the explanation of their nature by a reference to the phenomena attending the transformations of organic compounds, excited by the action of other compounds simultaneously undergoing analogous transformations. Thus yeast, which is gluten in a state of transformation, added to wort which contains gluten and sugar, converts the gluten of the wort into yeast, and at the same time the sugar into alcohol and carbonic acid, the two transformations going on together, and the latter ceasing when the former ends. The yeast reproduces yeast, if gluten, from which it was originally derived, be present; and, if the temperature and circumstances be favourable, fermentation may be spontaneous.*

* See Liebig's luminous exposition of the doctrines of fermentation, in his *Chemistry of Agriculture, Physiology, and Pathology*, 2 vols.

It must be admitted, with respect to all the forms of these diseases, that the body in the cycle of external circumstances through which it passes, may run into them spontaneously (in this they differ from the class of diseases referred to external causes), for it is impossible to trace them invariably to infectious sources ; it is not *à priori* more improbable that they, than other diseases, should arise spontaneously, and it is impossible to account for their existence in the world upon any other principle than that of spontaneous origin. Still the property of communicating their action, and affecting analogous transformations in other bodies, is as important as it is characteristic in these diseases which it is proposed therefore to call in this sense zymotic.* A single word such as zymotics is required to replace in composition the long periphrasis ‘epidemic, endemic, and contagious diseases ;’ with a new name and a definition of the kind of pathological process which the name is intended to indicate. Persons who have not made

* From ζυμωω, I ferment : zymosis, fermentation, and zyma, ferment, may also be employed in English, not in the sense which they have in Greek, but as general designations of the morbid processes and their excitors. Zymosis, and the verb from which it is derived occur in Hippocrates. See a good note and quotation from Galen, by Fæsius, in the *Œconomia Hippocratis*, appended to the Geneva edition (1662), of the works of Hippocrates. Coction appears to have been used by the father of medicine, with the same qualification as ebullition and fermentation by Sydenham. See his treatise on Ancient Medicine, vol. i, *Œuvres Complètes de Hippocrate*, par E. Littré, 1839.

themselves acquainted with the researches of modern chemistry, scarcely fall into the gross error of considering this peculiar kind of diseased action and vinous fermentation absolutely identical ; or of considering that others entertain that opinion. Liebig draws a distinction between fermentation and putrefaction ; the reasons are more urgent for distinguishing the pathological transformations from putrefaction or fermentation, while it is admitted that they are of a chemical nature and analogous to fermentation ; by which they are moreover to a certain extent explained, although so little is known of the series of chemical changes and products in any single zymotic malady, or of the chemical reactions of the living forces and organs. Small-pox is by hypothesis the transformation of varioline, and certain unknown concomitant chemical changes in the blood and skin ; manifesting the important symptoms which fall under direct observation.

Some of the morbid principles are fixed ; others are volatile ; but the greater part of them are fixed and volatile in different circumstances. Neousine, pestine, syphiline, lyssine, equinine, and vaccinine, are the most frequently fixed ; they give rise when placed on the skin, particularly where the epidermis is removed, to their peculiar diseases ; but contagion is not invariably the result of their contact ; indeed, in several of them it is the exception rather than the rule. Either there is no matter in the

organization susceptible of transformation, or the specific transformation is overpowered by the vital energies ; for in every case if the morbid principle (zymine) tends to impart its movement to the organization, the organization animated by the natural forces, has a tendency to continue its own processes, and to impart its conservative movements to all the organic matters which are brought within its sphere.

Varioline is converted in the cow (as Mr. Cecly has shown) into vaccinine, and cow-pox affords an interesting illustration of the modifications which diseases undergo, and which may be imparted to them by changes in their excitors. Vaccinine taken from the cow effects the transformation of the materies morbi in man almost as completely as varioline ; but it reproduces vaccinine ; and in the process is never fatal, never produces the variolous fever, and its vapour is never infectious like that of varioline. The mild form of small-pox which appears in persons modified by previous vaccination, or which follows small-pox inoculation, is an equally good example of the changes induced in diseases by the actual constitution of the individual, and the mode of infection.

Syphilis, erysipelas, necusia, metria, rubcola, scarlatina, and the other zymotic diseases, also put on different forms ; which may be referred to the state of the exciter, the mode of its application, the

matter on which the exciter acts, or the vitality of the patient. A modification of cholérine or of enterine probably produces diarrhœa. Louis considers dothineria (his *fièvre typhoïde*) a different disease from the typhus of this country, and points out the ulcerations particularly of the glands of Peyer, with the correlative phenomena, and the rose spots disappearing under pressure, as establishing its distinct character.* The differences in certain cases are unquestionable, and may be expressed by dothineria and typhus; but the two forms of the disease occur in this country; the characters are frequently mixed; and they are not greater than are observed in scarlatina simplex, and scarlatina maligna, with black incrustations and gangrenous inflammations of the throat,—in the erythema and phlegmonous erysipelas of Mr. Lawrence, or in the varieties of other diseases.

The blood which pervades the whole system is the primary seat of zymotic diseases; but this does not diminish the importance of the local phenomena with which they commence, proceed, or terminate; for they affect (as poisons do) particular organs more extensively and frequently than others, give rise to specific pathological formations or secretions, and derive their character from the lesions and affected organs.

The heat disengaged in these diseases suggested the term fever, derived from *ferveo*, as *fermentum* is from *fervimentum*.

Some zymotic diseases recur, others happen only once in life, or if they happen twice, it is the exception ; this has been explained on the hypothesis that some but not all kinds of matter (*zymen*) are reproduced in the organization after they have been destroyed by transformation (*zymosis*) in attacks of disease.

The tendency of zymotic diseases to increase and decline in activity, is one of their most remarkable properties ; and the suddenness of their outbreaks, with the great mortality of which they were the cause, excited at an early period the attention and solicitude of mankind. This tendency is indicated by the terms epidemic and endemic ; the latter serving to designate diseases which are excited by miasmata, and prevail in proportion to the quantity of miasm developed ; the former, epidemic, denoting the diseases transmitted from man to man independently of locality or only dependent on locality, temperature, and moisture as adventitious circumstances. For statistical purposes, the epidemic, endemic, and contagious diseases have been elassed under one head, as they may all be excited by organic matter in a state of pathological transformation. Ague is not contagious, and is apt to recur ; it therefore apparently approaches the class

of toxical diseases ; but I feel inclined rather to consider it a zymotic disease, in which to use the language of Liebig, the exciter is destroyed as soon as it is reproduced ; and this view is confirmed by the analogies of remittent fever, or yellow fever, so intimately allied in some respects with ague, in others with plague, and apparently contagious (though this is disputed) in certain circumstances.

Scurvy is a transformation induced by the want or inadequate supply of vegetable food. It formerly decimated the English navy, and is now met with in certain prisons. Scabies and porrigo (both contagious diseases) are ascribed to an insect, (*acarus scabiei*) and a low form of independent organization. The mode in which zymotic diseases are propagated has offered the ground of an interesting comparison between their diffusion, blight of vegetables, and the generation of animalcules."

Such is the view of zymosis as defined by one of its most eminent exponents, Dr. Farr. I have given his views in all their fulness, that nothing of them might be lost, nor any of them distorted. In my next chapter I shall indicate the modifications of the zymotic hypothesis which have been advanced by different observers.

CHAPTER IV.

SUMMARY OF VARIOUS AND SPECIAL VIEWS ENTERTAINED
BY MODERN AUTHORITIES RESPECTING MALARIA.

(CONTINUED.)

*Hypothesis that Malarious Poisons are organic. Views of Pettenkofer,
Snow, Grainger, Riecke, Routh, and Richardson.*

All the views which have up to this time been expressed, have been based on the idea of a cell origin or germ. I shall endeavour in this chapter to present a modification of this hypothesis, a modification which has been offered by various authors, and which while it considers all miasmatic poisons as organic in character, looks on them not as poisons that destroy by their own actual development, but as bodies which act as ferments, and by which new products of decomposition are brought about, the products acting in their turn as poisons, either directly or indirectly.

We have an illustration of this position of the question in the opinions of Pettenkofer on the origin and spread of cholera. Snow and others assume

that the poison of cholera, acting through its vital properties as a cell, is actually conveyed into the stomach of the affected person, where it undergoes increase, and by its increase destroys life, robbing the blood of its water by absorption and elimination. But Pettenkofer also tracing the cause of cholera back to the organic excreta of cholera patients, opines that this organic matter acts as a leaven on an impure soil in the neighbourhood of human dwellings, and that the special poison of cholera is a miasm produced by such fermentation. Mr. Grainger in a paper on animal effluvia and their effects on health, remarks that in order to obtain a right conception of this question, a knowledge of the general influence of putrid matter in the system is desirable. He shows that there are three modes by which animal effluvia may enter the body, namely, by inoculation, by the alimentary canal, and by inhalation. The effluvia producing specific diseases must, in his opinion, always be distinguished from the fetid gases (products of decomposition) by which these effluvia are for the most part accompanied. These gaseous matters themselves are often poisonous, and may destroy life, but if they do so it is in a certain recognized manner peculiar to each, and distinct from death connected with the class of agents called animal effluvia. Fourcroy and Berzelius, equally with Riecke and Tardieu, have recognized this distinction and its importance.

Mr. Grainger further infers that effluvium consists of organic matter, a protein compound in a most minutely divided form, and therefore adapted, as we know from the action of gaseous poisons, in a special manner to enter the blood, and there to diffuse itself rapidly and extensively. These subtle poisons are therefore material and organic; they are produced under certain known conditions and from certain sources, and there is therefore no reason why, as our means of investigation become more perfect, the exact nature, operation, and laws of effluvia, should not be as exactly known as those of any other class of substances. Continuing his argument further, this author points out the influence of the dissecting-room poison, and shows that in the dissecting-room there must be a poison afloat which sets up the specific diarrhoeal symptoms incident to students of anatomy: and he goes on to connect this fact with the symptoms which have been known to arise from the exhumation of dead bodies.

Mr. Grainger's remarks were published in the Transactions of the Epidemiological Society: in the same volume there is a short paper by Dr. Riecke, containing an epitome of his views on epidemics and contagion, which runs as follows:—

“1. All contagia,* under certain circumstances, have a primary origin; and even syphilis may

* Contagia—short for contagious matters; the principle of contagions.

be primarily produced. This is a fundamental principle.

2. In this process the mucous membranes play the most important part ; the volatile contagium especially is generated and secreted by them.

3. The contagia differ in their effect, intensity, etc., but they are identical in certain respects.

4. The part which contagia play in epidemics is, that they only contribute to increase the number of seizures in the limited focus (nidus) of disease, but that they are unable to diffuse a malady over countries and nations.

5. There is no such thing, and never has been, as a contagium capable of communication by the mere contact of the hand with the patient.

6. Epidemics arise in causes which are inherent in the terrestrial body. These produce the disposition in the living organism, and the so-called exciting causes (amongst which the contagium itself is included) induce the eruption of the malady. It is not until this stage that the contagium is generated by the morbid process.

7. There is no contagium which is capable of spreading a disease within a few months over the globe,—not even the plague and the small-pox.

8. The so-called miasmatic contagium arises in individuals disposed to it, if the predisposition is much developed. If many highly predisposed individuals are shut up in close, ill-ventilated rooms,

the malady originates primarily among them ; they, as it were, poison themselves. For this reason persons living in such places who are only slightly attacked become dangerously affected ; and the intensity of the contagium attains such a degree in such localities, that persons are affected whose predisposition is only slight."

Dr. Routh in a series of papers in the British Medical Journal, 1856, takes views somewhat similar to those advanced by Mr. Grainger, and shows that in fermenting excreta there is not only carbonic acid evolved, nitrogen, hydrogen, carburetted hydrogen, oxygen, and sulphuretted hydrogen, but sometimes when the fermentation proceeds to putrefaction, as shown by Lehmann and Frerichs, other substances precisely similar to those obtained by Bopp from putrefying protein bodies. Infection through alvine matters, according to Dr. Routh, occurs in three ways,—1, when such matters are absorbed in their natural concentrated state : 2, in solution or after suspension in water : 3, when the emanations therefrom are inspired. But in order that infection should be propagated three conditions are also required,—1, an infecting source : 2, a transmitting medium : 3, a fit recipient. These conditions he thus describes :—

1. " The infecting source may be the sick person, his fomites, excreta, etc. These sources are always

more concentrated and potent at the beginning of an epidemic, weaker at the end.

2. The transmitting medium may be (a) liquid, as serum or blood, as in inoculation; or water in which poison is suspended or dissolved, and taken into the body; or it may be (b) gaseous air containing a volatile substance. The thermometrical and barometrical changes which influence the gaseous infection may be the reverse of those which influence the liquid; the gaseous being more energetic in hot or moist weather, the liquid in temperate and dry weather; because in the first case the air is lighter, and the ammonia (which helps to suspend volatile poisons in the air, and to raise them to the nose,) exists in larger quantity. Hence the foul smells of drains are more sensibly experienced at such periods; whereas in moderately cold and dry weather the air is heavier, less ammonia is generated, and the poisons are kept nearer the surface of the earth.

3. There must be fit recipients. The fitness as such will be modified by race, as in the case of a negro, who is less susceptible to fever than a white man; by species in animals as is illustrated in the fact that the dog is innocuous to inoculation with plague matter; by concentration of the poison, which will infect the same man by its greater potency at the beginning of an epidemic rather than at the end; by acclimatisation, the effects of which

are seen in a yellow fever district, where the old residents suffer less than the new comers ; by previous disease, as instanced in the well known fact that a disease may sometimes not be taken more than once ; by occupation, which renders some men more exposed than others ; by state of health, and by food supplied. These are all modifying causes, and if they are not duly attended to, the greatest confusion results in the observation of an epidemic."

Regarding emanations from alvine evacuations as poisons, Dr. Routh takes the following views :—

There are three admitted conditions necessary for the emanation of miasmata,—1, a certain amount of temperature ; 2, moisture in the air ; 3, the evolution of ammonia. An opposite state leads to deposition on the surface or in water. Emanations are not possible below 32°F. The ammonia if formed then at all, will keep to the surface, and the same is true of watery vapour. A cubic inch of air at zero F. can only contain .856 grs. of moisture, while at 95°F. it contains 17.009 grs. The amount contained at 11°F. is almost 0, while at 50°F. there are 2.5. grs. In dry fine weather, especially if cold, drains do not smell ; in damp weather they are very offensive. So true it is that dampness often exists with epidemics, that Dr. Barton, of the United States, has founded thereon a theory which has been supported by Dr. Hunt, of Buffalo, (Transactions of the American Association, 1854), that

without the conjunction of dampness and what he has called terrene causes, no epidemic can occur. Terrene causes are any causes which may give rise to miasmata, such as upheaval of soil, and decay of organic matter. But besides watery vapour and ammonia, there are probably other gases evolved, such as carburetted hydrogen, sulphuretted hydrogen, phosphuretted hydrogen, and carbonic acid. In the decomposition of alvine matters there are two kinds of compound gases—in one, oxygen, 14 ; nitrogen, 81 ; carbonic acid, 2 ; and sulphuretted hydrogen, 3 :—in the other, oxygen, 2 ; carbonic acid, 4 ; nitrogen, 94 ; with more or less ammonia. The second kills because it is irrespirable ; the first because it contains an excess of sulphuretted hydrogen. The symptoms produced by phosphuretted hydrogen and carburetted hydrogen are not clearly made out, particularly when small quantities only are inspired. The symptoms produced by the inspiration of sulphuretted hydrogen in small quantities, are those of gradual prostration of the physical powers, general debility, followed by emaciation and low fever : generally, too, all diseases if long exposed to sulphuretted hydrogen emanations assume a typhoid and putrid character. The temperature at which these diseases develop themselves varies probably with each disease ; but, the ferment is destroyed at very high as well as at very low temperatures. Yellow fever ranges

between the temperatures of 60° and 90° . But there can be no doubt that something more is required, and this is to be found in the electrical state of the atmosphere. A positive electrical state of the atmosphere favours decomposition, and therefore disease in man. The same state of electricity favours vegetation ; hence, perhaps, the concurrence of very active vegetation in pestiferous districts. As vapours ascend so does positive electricity. This effect is less powerful in dry air. In some cases the electricity of the animal body is reversed. In health the mucous membrane gives negative electricity, and the skin positive ; in typhus these conditions are reversed.

The last development of the zymotic theory is the one advanced by Dr. Richardson, first in his paper on Zymosis, in the Transactions of the Epidemiological Society, and since in the first volume of the *Asclepiad*. According to this author, the zymotic diseases, while they are all produced by ferments, owe their specific symptoms not to the increase of the ferment, but to new and determinate compounds generated in the blood by the presence of the ferment. For example, he urges that the symptoms of scarlet fever are produced by an acid analagous to the lactic, which acid is according to his view generated in the blood of susceptible persons, by the introduction of an organic substance, derived from a person already infected, possessing

the power of exciting in blood this specific form of fermentation. Hence, zymotic diseases have a natural tendency to recover, because as the exciting poisons act only to a certain extent in producing fermentation, so the products are limited, and the symptoms remain but for such time as the products are retained in the body.

CHAPTER V.

ON HYPOTHESES RESPECTING THE ORIGIN OR DEVELOPMENT OF EPIDEMIC DISEASES, FROM GEOLOGICAL, METEOROLOGICAL, AND CLIMATIC INFLUENCES.

Views of Sir Ranald Martin, M. Boudin, Farr, Tripe, Richardson, and Keith Johnston.

While to certain observers the inorganic origin of diseases of the epidemic class, and to others the organic origin, have afforded explanations of the phenomena amply sufficient; to others these explanations have appeared altogether ineffective and unsupported by any demonstrative evidence. As a result, certain of the disbelievers have cast explanations of their own, which have related to the nature of those influences which act on the body from without, by the mere matter of change in respect of air, soil, and season. It is necessary therefore to examine briefly the opinions of these observers, since if we do not agree with them we must give them fair expression, assured that though they may not be right in respect to first principles, they may have in their arguments some large share of truth and reason.

Sir Ranald Martin, in his work on Tropical Climates, expresses a belief that the theory of the vegetable or marshy origin of malaria cannot be sustained, and from his experience he gathers that the geological nature of a locality is of the greatest importance in regard to the production of malaria, whatever they may be. He thinks that a highly ferruginous soil and a high temperature are the leading elements in the production of malarious influences. His remarks in this respect are deserving of considerable importance, as they are based on his own personal knowledge of India and its diseases, as well as on the experience of other observers in Africa and the Arkansas. M. Boudin seems to entertain a somewhat similar opinion. Sir R. Martin takes also into consideration another element, namely, the electrical conditions of the atmosphere. Animals, he says, waste and perish when they have been deprived of their positive electricity, by being attached to the opposite pole of a galvanic battery; and when the human body has been exposed for a long time to an atmosphere of negative electricity, it is believed by many to become thus incapable of resisting the various causes of disease—as exhalations from the earth, and epidemic influences. It is clear however that in this argument Sir. R. Martin recognizes a predisposing not a specific cause of disease, that is to say, not a malaria.

M. Boudin in his treatise on Medical Geography and Statistics, and Endemic Maladies, enters in some measure like Sir R. Martin, into the geological question regarding epidemics, coupling with it some meteorological observations. Diseases he says, in a similar way to plants, have their habitation, their stations, and their geographical boundaries. Thus cholera is found in Archangel, about 64° N. lat., but has never yet passed to Iceland, Greenland, or Siberia. It has ascended up to Canada in America, and has attained its southern degree in 21° S. lat. The Cape of Good Hope and Australia have as yet escaped its scourge. The limit of marsh fevers, he continues, may be represented by the Isothermal curve of 15° centigrade. The north of Scotland, the Hebrides, the Orkneys, the Shetland and the Faroe Islands, have thus been free from these fevers. In the southern hemisphere the limit of marsh fever has never yet reached to the Isothermal line of 15° centigrade. Yellow fever has never passed the 48° N. lat., nor the 27° S. lat. Its locality is bounded by the Gulf of Mexico, and the Caribbean Sea, and along the American coast to the Pacific. The plague has its eastern limitation in a line drawn from the Gulf of Mexico and extending to the Caribbean Sea.

Boudin also points out that elevation has a special action in regard to spreading diseases; thus marsh fever as it recedes from the Equator or from

the summer season, loses its type of continuity, and takes on the intermittent form ; and in like manner in hot and marshy countries, in proportion to elevation, these fevers pass gradually from the continued to the intermittent varieties. In like manner, in Mexico yellow fever never ascends higher than 924 metres.

Pushing his argument further, this author traces a connection between the soil and certain diseases, and argues that in many points in the United States and in Switzerland, marsh fever has passed away by desiccation of the soil, but has been followed by the appearance or multiplication of cases of pulmonary consumption.

Somewhat in a similar train, but in relation to our home diseases, Drs. Farr, Richardson, and Tripe, have pointed out the connection between seasons and certain disorders ; although they are I believe all acceptants of the zymotic hypothesis. Dr. Tripe's observations gathered from the statistics of the Registrar General, and collected, without any bias, from absolute facts, run as follows :—

1. Small-pox presents two periods of depressed and two of elevated mortality ; the first period of elevation occurring in January, and the second at the end of May and early in June, the former being the highest ; the first period of depression being at the end of March or early in April, and the second

in September, the latter being the lowest ; and therefore small-pox is most fatal in winter, and next in summer, and least fatal in spring.

2. A series of cold springs is attended with comparatively a large mortality ; and the period of highest mortality coincided during these years with a temperature of less than 40° F., and the lowest of above 46° F.

3. The average highest mortality, in seventeen corresponding weeks of 1840—56, was more than double the average lowest mortality.

4. Measles has only one period of highest and one of lowest mortality, the former occurring in December, and the latter varying in different years ; but the rate of death in spring is much smaller than in any other quarter.

5. The rate of death was greater in the series of spring and winter quarters which were below the average temperature, than in those which were above it.

6. As in small-pox, the average highest mortality from measles during seventeen corresponding weeks in 1840—56 was more than double the average lowest mortality.

7. Scarlet fever presents one period of highest and one of lowest rate of death, and this more markedly than either small-pox or measles ; the greatest number of deaths occurring at the end of October or beginning of November, the lowest in the middle or end of March or early in April.

8. The average mortality is higher in warm springs than in cold.

9. The mean of the greatest number of deaths from scarlet fever in seventeen corresponding weeks of 1840—56 was about twice and four-fifths as large as the mean of the lowest number of deaths in seventeen corresponding weeks.

10. Although there is a period at which fever is more rife than at any other—viz., from the thirty-sixth to the fifty-first week—yet there is not any particular short period at which it can be said to reach its culminating point.

11. The greatest rate of death occurs in autumn, and the smallest in spring: the difference between them being less than in any other eruptive fever.

12. The average maximum number of deaths in seventeen corresponding weeks in the years 1840—56 was not much more than one and a half that of the minimum.

13. The period of greatest mortality from diarrhoea is very definitely marked, occupying a period of about seven weeks (from the thirty-first to the thirty-seventh), during which more than two-fifths of the annual mortality takes place, the greatest rate of death extending from the last week of July to the first week in September.

14. That this period corresponds with a mean weekly temperature of 60° F. or above.*

* Sanitary Review, vol. iii., Transactions of Epidemiological Society, p. 37.

Dr. Richardson in his paper entitled "The Season; its Diseases and their prevention," taking the same ground as Dr. Tripe, gives the following remarks:—

"The analysis includes deductions made from not fewer than 139,318 deaths, occurring during years extending from 1838 to 1853, and arising from the following diseases: small-pox, measles, scarlet fever, hooping cough, croup, diarrhœa, dysentery, cholera, influenza, ague, remittent fever, typhus, erysipelas, quinsy, bronchitis, jaundice, and carbuncle. The districts of deaths were London, Devon, and Cornwall.

Out of the 139,318 cases thus chronicled as occurring from the above-named diseases, the percentage of mortality in the different quarters, and estimating the gross mortality according to the season, without reference to particular years, ran as follows:—

| | | | |
|------------------------------------|-----|-----|--------------|
| In January, February, and March | ... | ... | 25 per cent. |
| In April, May, and June | ... | ... | 21 „ |
| In July, August, and September | ... | ... | 24 „ |
| In October, November, and December | ... | ... | 28 „ |

Having learned thus much I set about ascertaining, on the same large scale, whether the fatal diseases were in any way special to the seasons. The answer to this enquiry is to this effect:—

Hooping cough, croup, small-pox, and bronchitis are most common to the first quarter. The percentage is:—

| | 1st quarter. | 2nd quarter. | 3rd quarter. | 4th quarter. |
|----------------|--------------|--------------|--------------|--------------|
| Small-pox ... | 27,352 | ... 24,551 | ... 22,824 | ... 25,272 |
| Hooping cough | 32,704 | ... 27,825 | ... 17,116 | ... 22,354 |
| Croup ... | 27,523 | ... 25,100 | ... 19,919 | ... 27,456 |
| Bronehitis ... | 36,793 | ... 20,301 | ... 10,327 | ... 32,570 |

Pneumonia I believe, might very properly have been added here.

In the second quarter, quinsy only stands ahead, thus :—

| | 1st quarter. | 2nd quarter. | 3rd quarter. | 4th quarter. |
|------------|--------------|--------------|--------------|--------------|
| Quinsy ... | 21,762 | ... 30,596 | ... 21,231 | ... 26,410 |

In the third quarter, diarrhœa, dysentery, and jaundice take the lead, in the following order :—

| | 1st quarter. | 2nd quarter. | 3rd quarter. | 4th quarter. |
|---------------|--------------|--------------|--------------|--------------|
| Diarrhœa ... | 10,196 | ... 10,717 | ... 58,519 | ... 20,567 |
| Dysentery ... | 15,638 | ... 13,541 | ... 42,460 | ... 28,340 |
| Jaundice ... | 24,877 | ... 24,030 | ... 26,967 | ... 24,109 |

In this third quarter, Asiatic cholera, when epidemic, assumes a greater mortality and prevalence than at any other season. Sporadic cases of cholera are, however, possibly more prevalent in the fourth quarter, during which influenza, ague, remittent fever, typhus, scarlet fever, measles, erysipelas, and carbuncle take the lead :—

| | 1st quarter. | 2nd quarter. | 3rd quarter. | 4th quarter. |
|-------------------|--------------|--------------|--------------|--------------|
| Influenza ... | 23,539 | ... 12,171 | ... 4,502 | ... 59,784 |
| Ague ... | 22,857 | ... 24,285 | ... 20,000 | ... 32,851 |
| Remittent fever | 23,077 | ... 26,315 | ... 23,481 | ... 27,125 |
| Typhus ... | 25,741 | ... 24,825 | ... 22,912 | ... 26,521 |
| Scarlet fever ... | 20,809 | ... 18,978 | ... 26,234 | ... 33,976 |
| Measles ... | 19,864 | ... 21,466 | ... 26,234 | ... 32,434 |
| Erysipelas ... | 25,144 | ... 23,444 | ... 22,337 | ... 29,174 |
| Carbuncle ... | 26,771 | ... 19,685 | ... 21,409 | ... 29,133 |

In a pathological as well as a statistical point of view, these results are most interesting: for they prove in a great measure, that diseases analogous in their general characters group themselves singularly together at special periods. Thus we see that, in the autumn quarter, there are grouped together those diseases which have for one of their essential symptoms an exudation from the intestinal surface, or that large abdominal viscus the liver. In the first quarter, the diseases of the respiratory system—croup, hooping cough, and bronchitis—stand forth prominently; while in the fourth quarter, a large family of diseases of the febrile or inflammatory order take the first position.

It is not by mere accident that these divisions occur: they are the effects of fixed, though nearly unknown physical or chemical laws.

It is worthy of special remark, that in the present quarter of the year—the fourth—the number of diseases which cause a prominent mortality is, as a general rule, greatest; and that next to it is the quarter commencing with the new year. During the third quarter of the present year, the mortality has been lower than is usual; a result due to the absence of any zymotic disease in a virulent form. As the cold of winter more decidedly sets in, we shall begin to see developed, almost of necessity, an increase of deaths from pulmonary diseases, and of low fever amongst the poor, if provisions

become high in price and insufficient in quantity or quality.”*

Mr. Alexander Keith Johnston, in his learned paper on the geographical distribution of disease, follows in much the same track as M. Boudin. He remarks :—

“The surface of the globe presents, with the utmost diversity of climate and soil, the greatest unity in its pathology. The same morbid appearances are reproduced with the utmost constancy and regularity in a thousand places at once, wherever the same causes of insalubrity are met with, only that they are more intense, continuous, and prolonged, in some places than in others.

Similarity of geological formation indicates a similarity in the diseases of a country, as seen in the localities visited by malarial fevers. A certain amount of heat, and a sufficient time for its manifestation, are necessary for the development of certain maladies. In the West Indies, the period of disease follows the course of the sun, the unhealthy season occurring at opposite times on the northern and southern sides of the equator. As the sun proceeds northwards in the ecliptic, so the sickly season advances from the southern to the northern islands. In the Mediterranean the mortality is doubled in the hot season, between July and October; and in the southern States of North America the

* Sanitary Review, vol. 1, p. 366.

posts of the army are regularly abandoned as the hot or sickly season approaches. But in temperate regions the order is reversed. Throughout Europe generally the maximum mortality occurs at the end of winter, and the minimum in the middle of summer. The Registrar-General of England calculates that a fall of the mean temperature of the air from 45° to 4° or 5° below the freezing point, destroys from 300 to 500 of the population of London. The agency of the wind is manifested in the distribution of heat and moisture, and in the comparative density of the air, as well as by its direct influence as a distributor of malarial poison; and the absence of wind was uniformly noted as a concomitant of cholera, which in Britain was always most virulent when the calm was greatest, and began to abate when the wind rose. During the great epidemics of Andalusia, 1809—19, the Levanter, a south and south-east wind, was observed to blow constantly for nine months in the year. During the prevalence of the Sirocco, on the Mediterranean shores of Asia and Africa, vaccination fails, as does also inoculation with small-pox, and ulcers and wounds are more difficult of cure.

As elevation above the surface causes a corresponding reduction in the temperature and in the pressure of the atmosphere, so those diseases which are prevalent at the level of the sea, in cold or temperate countries, are found to be represented by the same, or similar ones, at elevated points

in tropical regions, where a corresponding low temperature prevails.

In tropical climates especially, electricity in its different forms is believed to have a powerful influence on the morbid affections of the human frame ; and during the cholera epidemic in Britain, the observations at Greenwich show that the atmosphere was always deficient in positive electricity when the disease was present.

REGIONS OF DISEASE CORRESPONDING WITH SEASONS AND ZONES OF CLIMATE.

The surface of the globe may be divided into belts or zones distinguished by great leading characteristics ; as 1, the torrid zone, or belt of greatest annual mean temperature, characterised by the class of diseases which includes dysentery, yellow fever, diarrhoea, malarial fevers, and affections of the liver ; 2, the sub-torrid and temperate zone, of which inflammatory diseases, represented by typhoid fevers, are the characteristic maladies ; and 3, the sub-temperate, sub-arctic, and arctic zone, characterised by catarrhs and colds.

1. The immediate dependence of the first class of diseases on heat and moisture, as important exciting causes, is shown by the circumstance that its maximum intensity corresponds with the countries situated under the line of greatest annual mean temperature, the assumed equator of heat of the

globe (82° Fahr.) ; which line also intersects the region of greatest aqueous deposition. From this line, to about latitude 23° north, 53 per cent of the deaths are attributable to this class of diseases ; while in latitude 35° north, marked nearly by the line of 77° Fahr. in July, and on the boundary of the second zone, the amount is only 14 per cent ; and at the Cape of Good Hope, latitude 35° south, it is only 3 per cent. As far as can be ascertained, the mortality from the entire classes within this zone amounts to 75 per cent,—the first and second causing 53, and the third 18 per cent of the whole. The same law of decrease with the lowering of temperature is apparent in the seasons of their occurrence. In a series of dysentery epidemics, narrated by Ozanan, 36 occurred at the end of summer, 12 in autumn, and only one in winter. Of 13,900 individuals seized with dysentery in Bengal, 7000 were attacked in the warm and humid season, 4500 during the hot and dry season, and 2400 during the cold season. In spring these diseases are more inflammatory in their character, and in autumn more putrid.

The northern limit of this class of diseases is probably the Bermudas, latitude 32° north, in the Atlantic ; and California, latitude 38° on the Pacific Ocean, in America. In Asia it extends to near Peking, latitude 40° north ; and in Europe to the south of Spain. Its southern limits are—in America,

Bucnos Ayres, latitude 34° south, on the Atlantic, where, however, it is not severe; and Lima, latitude 12° south, on the Pacific. In Asia the southern limit includes Aracan, Ava, and Ceylon, Borneo and the other islands of the Asiatic Archipelago, and thence it extends to the northern shores of Australia. In Africa it includes the island of Madagascar. Within these limits the principal centres of these diseases are,—in America—the shores of the Gulf of Mexico, the West India Islands, and the northern portion of South America; in Asia—India, China, Borneo, Ceylon; in Africa—the countries around the Gulf of Guinea on the west, Madagascar and Mozambique on the east, Algeria and the shores and islands of the Mediterranean on the north. Little is known of the perpendicular distribution of these diseases, except that in Mexico they are prevalent at an elevation of 7000 or 8000 feet; and in south-eastern Asia they cease at an elevation of 6000 or 7000 feet above the sea.

2. In the inflammatory region, or zone, typhus fever in its varied forms of gastric, bilious, enteric, &c., fever, takes the place of the yellow and malarial fevers of the torrid zone; and in consequence of fewer of the population being cut off with these, more fall victims to inflammatory affections, of which consumption is the type. But that this latter form of disease is not peculiar to this region,

or rather that it becomes more fatal as we approach the tropics, is proved by the fact that in England consumption is only fatal to 3.8 out of every thousand living, while Boston (U.S.) loses 4.0, Baltimore 4.1, Philadelphia 4.2, New York 4.9, and New Orleans 5.6, out of every thousand living.

In North America and Europe, the southern boundary of this group of diseases coincides generally with the northern boundary of the first class. In South America it probably includes Patagonia. In Africa it includes the Cape Colony; and it embraces the South of Australia, Tasmania, and New Zealand. In Asia it is uncertain how far it extends to the eastward. Its northern limit in America includes part of Nova Scotia and Newfoundland; and in Europe the northern boundary includes the British Islands, Norway, and Sweden, to latitude 60° north, whence it appears to follow a south-eastern direction, corresponding nearly with the annual isotherm of 41° , till it gradually declines towards the borders of Asiatic Russia. These, however, are only to be considered as preliminary indications.

3. The boundaries of this group of diseases, which is characterised by catarrhs, include the whole of Europe to the north of the preceding class. In America it extends south to Boston and New York, including the district of the Canadian Lakes. Thence it continues north-west nearly on the line

of 41° annual temperature. Although very little is known of the diseases of Central Asia, yet, when we consider the elevation of the surface, the vegetation and the conditions of climate, we may assume that this class of diseases extends there to about latitude 45° . Iceland is the best-known locality of this zone, and may therefore be taken as its representative. The island is visited by catarrh every year in spring or in early summer. It is also visited at short intervals by catarrhal fevers,—a true influenza, which usually has a great effect on the mortality. Pallas says that the majority of Icelanders die before the age of fifty, from asthmatic or catarrhal affections of the lungs ; and Crantz affirms that catarrh is a very prevalent disease in Greenland. Catarrh is also common in Labrador. At Okhotsk in Siberia, it is accompanied with difficulty of breathing ; and a cough, called “Ho,” is endemic among the Samoeids.”

CHAPTER VI.

SUMMARY OF OPINIONS.

It was my intention in commencing this work, to have endeavoured to give a complete historical survey of the various doctrines which have been held in regard to malaria and miasmata, their nature and their effects. But the labour would require a lifetime, and could scarcely be embodied, when completed, in a hundred octavos. More than that, it is in some measure unprofitable labour, as it elicits no connected lines of observation, no analogies of opinion, few corroborations of phenomena, few experimental works of a solid kind, and no hypotheses which have lasted for any continued period. Instead therefore of offering the hundred volumes, and the work of a long life, I shall try and condense into a few paragraphs what, from my reading, seems to be the pith of that which has been said and done, in reference to the diseases now under consideration.

1. There is the oldest of the old supernatural theories, which Celsus describes as the theories of

those who attribute diseases to the anger of the immortal gods. We need not dwell on these.

2. We have the theory which supposes pestilences to arise from grand cosmical causes, attributing them to earthquakes, emanations from volcanoes, the approach of comets, falling stars, great perturbations of the atmosphere, and the like.

3. There have been enumerated the various forms of malaria or miasmata, and these again have been variously divided and commented on. Some have considered a malarious influence as something unknown and undiscoverable ; others that malarious or miasmatic influences spring up as independent poisons, and contaminating an atmosphere, affect large numbers of individuals at the same time ; another body of argumentatists have urged that malaria are in themselves simple agents, perhaps always present in the atmosphere, but are brought at various times into greater activity by the condition of the atmosphere or by meteorological influences ; others have supposed that malaria are simple and perhaps well-known substances, that they are always present, but that for their action as poisons certain modifications or predispositions of body are demanded ; some have believed that there are as many specific malarious agents as there are specific diseases ; others have disputed this, and have supposed that the poisons which produce typhus and ague are really the same, but are

modified in their effects, either by climate and season, or by peculiarity of constitution. Another class opine that there may be a few specific poisons, and that modifications of disease may occur from combinations of these, in like manner as a few chemical agents may by combinations play many parts and produce innumerable compounds.

4. The nature of malarious agents has been differently discussed by those who hold the theory of the presence of malarious matters. Some men have considered malarious agents to be matters of organic life, that is to say, invisible cells, germs, fungi, or animalcules generated under favouring circumstances, caught up by the atmosphere and conveyed to man through the respiration, or communicated to him in the soluble form by inoculation. They who espouse this view, as a primitive view in respect to cause, differ widely as to the course and consequence of the origin. One sect assumes that the germ introduced into the living organism is actually multiplied there: others think that the poison being organic primitively, does not necessarily act on the affected organism directly, but that coming into contact with decomposing matters in the soil, it gives rise as a ferment to the formation of compounds which become poisonous when breathed, malaria. A third class opine that the organic poison introduced into the blood, and favoured in its transmission into the blood by heat and moisture,

has the power after its absorption, of exciting zymosis and producing decomposition of the blood itself, and of the tissues which are dependent on it for nutrition, of thus setting up an entire modification in the chemistry of life, and of inducing those departures from the healthy standard which we call, under different names, specific diseases.

As to the method by which these changes are induced, but little has been advanced except by one author, who, as we have seen, argues that the symptoms of disease are not produced immediately by the organic poison, but are the result of the formation of a new poison, the product of the fermentation which the organic substance has called into action.

5. There is the argument that diseases originate in the body itself, irrespectively altogether in the first instance, of external conditions; that a body thus affected may, however, generate a specific poison, which, whether organic or inorganic, emanates from it, and coming in contact with other bodies gives rise to the same disease. This is the modernized doctrine of contagion; it presumes that disease can only be propagated by direct or indirect intercommunication.

Lastly.—There is the view that certain meteorological and geological conditions are intimately and positively connected with the origin and spread of diseases of the epidemic and endemic class. The

advocates of this view again divide themselves into two classes, the one class believing that the external changes are sufficient per se to produce the particular disorders, the other urging that the external conditions do not actually produce the malady, but that they call into play the active properties of the poisonous agents, which are the real source of the disease.

It has been remarked by Lewes that in all scientific pursuits there are three grand historical series or ages. The supernatural age in which men refer the simple phenomena of nature to supernatural causes, that is to say, to the anger of offended deities, or special spirits presiding over the universe. Next there is the metaphysical age, in which certain ruling powers or mystical entities are assumed, and named as governing the natural phenomena, but indefinable in themselves; named by such general terms for example as miasmata or malaria, or predispositions, or occult causes. Thirdly, there is the true scientific or positive age, in which men endeavour to bring every thing to the comprehension of the senses, to throw overboard all that is supernatural or metaphysical, to present only such explanations of phenomena as admit of being proved by actual experiment or repeated observation, and to place everything on the basis of positive and natural truth.

In the study of epidemic diseases these three stages of scientific inquiry are forcibly presented,

and we are happily in this day endeavouring to solve the problem before us by strict application to the last or positive method.

Such are some of the theories which have been advanced regarding the nature and diffusion of malarious poisons. From theory it is a curious historical lesson to turn to the various facts, real or presumed, on which the theories thus recorded have been based. A great work lay before me in this direction, and I entertained the project of bringing into descriptive form a list of the best marked instances in which diseases of a special type appeared to be introduced and propagated by general causes traceable as general causes if not defined according to their absolute nature. After commencing this work, and after collecting extensive material for its illustration, I have given it up from a comprchension, on a closer acquaintance with it, of its entire uselessness. It is true there are to be found isolated examples in which diseases seem to have arisen in healthy localities specifically from the inhalation of a volatile poison in the air. But out of a hundred presumed illustrations of this character, ninety are so confused and indefinite, and so mixed up with other circumstances which might be causes, that no facts according to my judgment can be deduced from them.

I have also been compelled to give up the analysis of the works of those who have supported the idea

of the dissemination of specific diseases apart from the notion of malaria, for these have so many powerful arguments on their side, that the two classes of combatants may well claim drawn battle. Therefore to enter into argument on one side would imply in fairness a similar labour on the other side, and would lead to the task of playing umpire in a contest in the which neither party has sustained a perfectly logical and demonstrative position.

Thus I reluctantly leave these vexed questions, and if, in after pages, I take a special view, I shall be led into it from my own independent reasonings and observations, not from bias towards either the advocate of malaria or his antipode.

At the end of the work will be found an alphabetical list of those authors from whom I have endeavoured to obtain such information as would guide me in the course before me. The reader who may be anxious to follow out the history of this subject in detail, will find in this table a tolerably complete index to his authorities, and will be able to judge whether or not the preceding abstract is a fair *resumé* of what has been written.

CHAPTER VII.

ON THE DIFFICULTIES IN THE WAY OF DISCOVERING
THE ORIGIN OF SPREADING DISEASES.

Causes of differences of opinion. Methods of enquiry. Epidemics and Endemics. Specific causes of different diseases. External origin of diseases considered. Poisons indestructible. Reproduction of poisons. Influences of external conditions. Meteorological influences. Light. Geological conditions. Mental influences. Nature of epidemic poisons. Summary.

The differences of opinion noticed in the last chapter, while they are in some measure attributable to an absence of the inductive principle in medical inquiries, are still more attributable to the numerous difficulties with which the question of the origin of diseases is necessarily complicated. Two methods of studying diseases have in fact obtained ; by one method an endeavour has been made to trace out causes first and then to follow up the diseases from their causes ; by the other method the aim and object has been to go back from the diseases themselves to the causes of them. The first is unquestionably the soundest proceeding, and at the same time the most difficult ; the second therefore has ever been the most popular, and at the same

time most delusive. If, with a known poison in our hands, it is hard and indeed impossible as yet to know the *modus operandi* of such poison, how much more difficult must it be to understand symptoms which arise from poisons only theoretically known, and perhaps not known at all.

The difficulties in our present subject are moreover exaggerated a hundred-fold, by the fact that we have a number of diseases to account for, all differing from each other, and yet all seeming to arise from a cause analogous in its general mode of operation, although different in its details. We use the word epidemic and the word endemic in truth to typify classes of diseases which from the facts that the diseases appear at certain times, run a certain stage, and take a certain specific type, seem to be under some one general controlling influence, and yet are so different in their characters, that we may logically suppose there are as many controlling influences as there are diseases. In this simple statement we see at once the reason of the many differences of thought which have arisen.

For my own part it appears clear that in studying this great subject, one of two courses must be taken; either we must not classify together endemic and epidemic diseases, that is to say, we must ignore the analogies which exist in these diseases, and take them into consideration as positive isolations, or we must ignore the idea of different causes, and suppose

that one cause modified in its mode of operation is at the root of all. Without endeavouring to lay down a dogma, I should conclude that the causes of the different diseases are specific, and alone admit of being isolated; while the analogies which exist between the diseases, only exist because the same general influences bring into force and activity those special agencies by which the special diseases are induced; ergo, there are as many causes of disease as there are diseases.

Again, with regard to the origin of the epidemic and endemic diseases, the inference seems equally clear that such origin must be external; for although some diseases seem to spread from person to person, other diseases of the epidemic and endemic classes, are evidently purely *ab extra*. Nor does the fact of a disease being communicable from one person to another, in any way show that the first instance of that disease was not from some external cause, a cause which may possibly have died out, but which, with equal reason, may be supposed as still extant. I infer indeed, that the cause of a disease may be external, may be received by an individual, may be communicated by that individual to others, and that others may thus receive it; but yet that the original cause may remain, and that still other individuals as well as the first may receive the disease from the original cause. In other words, I presume that a poison

may be received into the body, may set up its specific effects, may pass from that body and be transmitted further. For if the poisons which give rise to a disease are indestructible in the animal laboratory, and if it be true, as all evidence shows, that the natural cure from poison consists in the elimination of the poison, it is equally true that the body of the animal is but the receptacle for the time of the poison itself, and that such poison is but laid by for the moment.

There is, moreover, this to be considered in regard to the poisons of some epidemic diseases:—that when the body of an individual is brought under their influence, it has the power of increasing the amount of the poison originally carried into it; of this we have a striking illustration in the disease small-pox, the virus of which introduced by a small spot into a living animal, generates a thousand fold; yet it is clear at the same time, that if a person is not exposed to the said poison, he is not subjected to the same disease.

I would lay down then the following views, subject to the experience of other observers; that the causes of the diseases we are now considering are primarily external; that the fact of their being communicable from person to person is in perfect accordance with their external origin; that the body in respect to some of these poisons has the power of generating them in abundance, and that while this

may be one means of conveying such poison, the same poison may be conveyed from the main source, that is to say, from the external source.

Further, it is not improbable that the influences which bring forth into full activity the effects of external agents as causes of disease, may also assist in the propagation of such diseases in animal bodies, and that the chemical or organic agencies which are at work in producing the poisons externally, are also at work in favouring the development of the poisons in animals subjected to their influence.

I have spoken here of the producing poisons of the epidemics, as poisons of an organic character. This may be open to objection, since it has been urged by some authorities, that certain inorganic volatile agents may be all-sufficient for the production of specific diseases. The inorganic poisons which have thus been specified are in the main, carbonic acid, carburetted hydrogen, sulphuretted hydrogen, sulphide of ammonium, and phosphuretted hydrogen. In regard to these agents, the fact of their poisonous character cannot be called into question; neither can it be disputed that some of them are always present in localities favourable to the development of spreading diseases.

But in relation to these poisons it requires further to be ascertained:—

1. Whether they have the property of exciting and sustaining a special disease throughout its course.

2. Whether their presence is not compatible with the idea that they are the mere attendants of another poison of the organic class ; adding perhaps by their presence to the intensity of such poison, but not exciting in themselves the specific malady, as specific causes.

In regard to the first of these questions, many unanswerable arguments lead to its negation. The first and most powerful of these arguments is the one, that the poisons which produce a spreading disorder, are reproduced in the animal body. In the second place, the specific poisons of some diseases such as small-pox, are in our hands, and we know they are not of the common inorganic class of poisons. Thirdly, we are certain that the spreading diseases run a certain specific course, which could not obtain if an inorganic poison, floating in the atmosphere, were the poisonous cause ; since, if that were the case, the disease should get well as soon as the poison was removed ; and on the contrary, the disease should not get well so long as the poison was present. Fourthly, in the presence of these diffused inorganic poisons, all should suffer alike in the locality in which they are confined, if they were the cause of spreading diseases ; but this is not the fact. Finally, we may negative the idea of some of these inorganic poisons being disease producers, by the absolute observation, that men are not unfrequently subjected to them

without evidencing any of the specific maladies. Miners inhale marsh-gas, and are sometimes poisoned with it by mere narcotism, but miners are not sufferers from ague, as they would be were this gas the product of decomposition in marshes, the marsh malaria,—and the efficient cause of marsh fever.

The answer to the second question proposed above, will be given more fully and experimentally in a succeeding chapter, and that answer will be found to be affirmative in kind ; for I shall have occasion to show, that even minute portions of some of these inorganic products have the property of producing very serious symptoms ; symptoms analogous in fact to certain forms of low fever. It is rational, therefore, to accept these poisons not only as coincidences, but as dangerous coincidences ; and to recognize in them, in reference to fevers arising from the reproductive poisons, the truth of the old Spanish proverb, that

“In trouble to be troubled
Is to have the trouble doubled.”

From this same point of view, meteorological changes come legitimately before our notice. Some writers have believed that meteorological conditions are alone sufficient to produce various epidemic and endemic diseases. But if this be the fact, it is quite clear that our present knowledge of meteorology is entirely wanting in some all-important particulars, inasmuch as up to this time, no definite

meteorological condition or conditions can be adduced as a reason for any specific disorder; while if this argument were admitted, all idea of the communicability of disease from person to person must be entirely thrown aside,—a proceeding which in respect to such diseases as small-pox and scarlet fever, is utterly unwarrantable. I shall not push this point further at the present moment,—because it is my intention, in the following chapter, to give a detailed account of two years' meteorological observations, with the view of showing from personal observation, what relations are to be traced between spreading diseases and meteorological changes.

Notwithstanding this statement, it is not to be denied that all epidemics are more or less under the indirect control of the grand meteorological phenomena of nature. With such facts before us regarding seasons, elevations of site, geographical distributions of disease, and the like, as were stated in the concluding part of the last chapter, it would be illogical to deny to such influences a share in the production of epidemics. They act however, as I shall endeavour to show, not as producers, or causes, but as influencing the propagation or suppression of the true causes.

The influence of light in the propagation of some epidemics, has been occasionally referred to. It has been stated that influenza selects the shaded

side of streets, as one example. The influence of light on the organic world is a scientific problem as yet in its infancy. We know that plants require light for the decomposition of the carbonic acid gas which they imbibe, and we might infer therefore, and may infer, that light is something more to man than a mere medium for bringing him into connection with the external world ; but its chemical influences are too subtle to be speculated on, and science will have to make many a long stride before the chemical effects of light can be subjected to rigid observation and demonstration.

The effect of geological influences in the spread of the epidemic diseases is doubtless of moment, for it is not to be disputed that particular geological formations are connected with the rapid spread of some diseases. But we must pause before we connect such formations with the absolute origin of a disease. For did we connect geological states with the direct origin of any malady, we ought certainly to find such malady limited strictly to the districts in which such geological conditions obtained. We find however, on the contrary, that no such rule can be established. Again, we must throw overboard under such an hypothesis, the idea of communicability of diseases, a sacrifice of truth for the sake of argument, which as I have already shown, is quite unjustifiable.

At the same time it would be unphilosophical to say that geological conditions exert no secondary influences. Sir Ranald Martin may be quite correct in regarding ferruginous soils as connected with the spread of particular disorders, but such connection must not be taken in relation to the origin of the diseases, but in regard to the connection which may subsist between the influence of such condition on the poison from which the disease originated. The relations which may exist in these respects may be obscure, and yet not less truly existent, while a knowledge of those relations may tend to elucidate the nature of the specific morbid agents themselves; but I contend and this is the only point I would argue for, that the idea of a specific cause of disease, in fact of a specific poison, remains unaltered.

Once more the influence of the mind has been adduced as explaining in some measure the diffusion of epidemic disease. I would not deny that mental influences, like geological and meteorological influences, may have their secondary effects, and they may so depress the body, that the action of a poison may become doubly increased in intensity; but that the mind by its forebodings or terrors, can alone summon up a specific disease such as small-pox, accompanied by external symptoms, and running a certain stage, is as absurd as to presume that the mind could produce the symptoms caused by strychnia, prussic acid, or any other known

poisonous agent. It is clear, indeed, that if the mind could direct the body into a disease, it could also direct the said disease through all its stages, and modify those stages according to its own will, which is the argumentum ad absurdum.

In these remarks regarding the effect of the mind, I exclude epidemics of a purely mental character—such as the dancing mania, or the enthusiasm or depression arising from religious or political fanaticism. These are not corporal diseases, but insanities; nor do they come under the category of diseases marked out in my programme.

To show the groundlessness of any hypothesis which should connect the mental phenomena with the production of epidemic diseases, we have only to refer to the epidemics of the inferior animals. There is no fact more marked in nature than that animals suffer from spreading diseases, while it is clear as crystal that here, at all events, the mental phenomena can have no share in the production of disease. An ox will see his fellow led to the death, will see him struck down, flayed, dressed, and yet show no more hesitation or resistance in going to the same end, than if the spectacle had not occurred before him. Yet an ox placed side by side with his fellow suffering from pleuro-pneumonia will take the disease and die from it,—not certainly from the mental phenomenon, but from the physical fact of the transmission of the specific cause of the disease.

It is often easy to illustrate the nature of an obscure phenomenon by the analogies of an obvious one. There is the disease hydrophobia, evidently specific in its symptoms, evidently communicable only in one way,—that is to say by communication, and by direct inoculation of the poison producing the disease. Hydrophobia however is more prevalent at one season than at others, ergo, it is in some way influenced in its progress by external conditions, which may be, and possibly are meteorological: but to argue that hydrophobia is the result of meteorological changes, or geological states, or mental influences, is too ridiculous to admit of a moment's discussion. Yet the disease in question affords an excellent guide to our studies of other communicable diseases. Why do we not take the coincidences which attend the propagation of hydrophobia as causes of that disease? The reason is obvious, because in a general way we know the origin of the disorder. The analogy is an extended one. It applies to all the other specific diseases, with the simple exception that we do not know the origin of the specific poison from which the other diseases are generated.

I do not think it necessary to carry this line of reasoning further. The thing is clear, as I shall have occasion to show in the sequel, that there are specific poisons for all the diseases under consideration, and that other agencies and influences,

whether meteorological, geological, or mental, are but secondary agencies, adding to the effect of the primary, and no more. It remains therefore for us to consider how the specific poisons are conveyed into the system, and what they are in themselves.

As regards the nature of these poisons, it is clear that they are reproductive under favouring circumstances, and that the human body affords a field in which such reproduction is well developed. In small-pox this is perhaps better shown than in any other disease. It is further clear that the poisons are diffusible, and that such diffusion may be carried on either by mechanical admixture with the animal fluids,—as when the poison is inoculated, or by the atmosphere, as when the poison is inhaled.

It is no argument whatever against the diffusion of these poisons by the atmosphere, that they have not as yet been detected in the atmosphere,—for the fact of the force of the poison does not rest on its amount as a poison, but on its power of reproduction when it finds a favourable medium. The minutest particle of glander-matter introduced into the body by the lancet-point, sets up the glander disease. It cannot matter whether this minute particle be received by a wound, or by absorption through a mucous surface; indeed we know it does not matter, for if the poison be laid on the mucous surface of the eye the result is the same. Therefore the result would be the same if

the poison fell on the mucous surface of the lungs. Diffuse then throughout the atmosphere as much glander-poison as would destroy a thousand men, and what means would we have for detecting that poison in a thousand cubic inches of air? None.

In late times an argument has been brought forward that some epidemic poisons, specially the poison of cholera, is diffused mainly through water. The argument as far as it goes, is conclusive, and the late Dr. Snow immortalized himself by showing this simple truth; but it is not sound argument to confine the choleraic, or any other poison to this one medium. For if these poisons are diffusible in water, they are as probably diffusible in air, under conditions favourable to such diffusion.

In limine, whoever will observe the mode in which epidemics are propagated, will I think learn the following simple truths, which for the sake of conciseness I shall reduce to a few separate sentences or rules.

1. Specific epidemic diseases are derived from specific poisons, poisons reproductive in the animal economy, and reproductive possibly out of the animal economy, under conditions favourable to reproduction.

2. Such specific poisons received into the blood, whether by a wound in the skin, or by absorption from mucous surface, produce the same specific effects.

3. The amount of poison is of little moment, when taken into consideration with the force of reproduction.

4. The poisons are diffusible only to a limited extent; for if they be gases, the diffusion process in the atmosphere disperses them; while if they be organic particles they float but for small distances, and remain active or inactive according to the medium with which they come in contact.

5. Meteorological influences do not assist in the diffusion of these poisons, but rather in the reproduction of them,—inasmuch as those conditions of a meteorological kind which are evidently connected with the prevalence of epidemics, such as high temperature, would rather favour the dispersion and removal of volatile poisons than their concentration and intensity.

6. As a consequence of the last-named position, matters on the earth exclusive of man, may be a constant storehouse of these poisonous agencies; the poisons may thus lie dormant for a time, like the seeds of a plant; at favourable seasons they may rise into full activity, and man subjected to their influence, may become the vehicle of their reproduction, and of their further transmission.

In the line of discussion thus pursued, I have endeavoured only to connect the observations of other observers with common-sense principles, and to indicate the causes by which so much disparity

of opinion has arisen. If it be true as I have presumed, that there are specific poisons for the diseases under consideration, and that external conditions, whether telluric, aqueous, or ærial, assist primarily in the reproduction of such poisons, and secondarily only, in their diffusion, the foregoing difficulties are at once mainly removed. Any way there are obviously two things to be considered, reproduction and diffusion; and it is as fair to dwell on the importance of the former as of the latter. We may speak indeed of this generation and migration of diseases, as we would of the generation and migration of men, animals, and plants. The *generation* of animals and plants is dependent purely on external favouring conditions, though the *origin* of animals and plants is purely in animals and plants. The migrations of animals and plants are accidental phenomena, so in like manner may be the migrations of the spreading disorders.

Taking it all in all, there can be no doubt that the evidence that has been collected by various observers, irregular though it may have been, and contradictory as it appears, is in favour of an organic origin of epidemic diseases, and of the view that a specific germ once received into the system, is there reproduced by some process as yet ill-defined. Against the fermentation or zymotic process it has, I know, been urged that the production of a sporule or germ in fermentation, is

as much a result of chemical change as is the generation of alcohol and carbonic acid under similar circumstances, and that the three products hold a distinct relation to each other. I know, too, it is said that if the development of a disease were really due to a zymotic process, then in the body in which such process was being developed, there should be, according to the fermentation doctrine, a generation of alcohol and of carbonic acid; facts which have not as yet been proved entirely, although Macgregor and others inform us, that in small-pox, the amount of carbonic acid exhaled by the breath is very much increased in quantity. How these disputations are to be reconciled remains for further inquiry; but this as it appears to me may be suggested, that a process going on within the body, although analogous to what occurs out of the body, may be modified in its details; that is to say, the products of the process may possibly be changed as they are produced, under the influence of the chemical forces in the animal laboratory. However this may be, it does not alter one iota the simple, but all-important truth, that a speck of small-pox virus introduced into the healthy blood is reproduced there a million-fold, and that this reproduction must be attributable to a germ which is susceptible of increase in the animal economy.

CHAPTER VIII.

ORIGINAL OBSERVATIONS ON THE RELATIONS OF VARIOUS
METEOROLOGICAL STATES TO PARTICULAR DISEASES.

Observations on the Author's district. Tests of instruments used in the inquiry. Barometer. Thermometers. Mode of observations and times. Diseases observed. General barometric readings and disease. Thermometric readings and disease. Season. Presence or absence of Ozone. Direction of wind. Conclusions. Geological notes.

The succeeding chapters contain the results of an inquiry, extending over two years, in respect to meteorological conditions and particular diseases. To make this inquiry perfect, I fitted up an observatory with every possible care. The instruments used were as follows:—

The barometer was made by Harris and Co., its readings were compared with those of a standard barometer, by Mr. McLaren. It had a brass rod extending from the surface of the mercury in the cistern; its readings were corrected for capillarity and for index error, and reduced to the constant temperature of 32° Fahr., by the application of the numbers supplied in the Report to the Royal Society of the Committee of Physics. The times of reading the barometer were 9 a.m. and 3 p.m. daily.

The dry and wet-bulb thermometers were made by Bennett. They were placed at a distance of five feet from the ground, and five feet from other objects. Their readings were compared with a standard thermometer by Mr. Glaisher, F.R.S. The times of reading were 9 a.m. and 3 p.m.

The self-registering thermometers were made by Jones. They were of Six's construction, were placed side by side with the dry and wet-bulb thermometers, and were read at 9 a.m. daily.

The rain-gauge was of the ordinary staff construction, and the distance of the receiving surface from the ground, 3ft. 6in.

The ozone test papers were those brought into use by Schönbein and Moffat. They were introduced into the box constructed for the purpose by Mr. Prince of Uckfield. This box was so made as to admit of a free current of air through it, but no light. The amount of ozone was recorded every morning at nine o'clock.

All these observations, as they were taken, were recorded accurately.

During the whole of the same period, the diseases surrounding the central point or observatory, for a space of five miles, were collected, and as I had unusual facilities for obtaining a knowledge of the existence of disease in this area, I can state positively, that no epidemic existed in it during the time without my knowledge of it; and that every such epidemic was recorded.

In so far therefore, as daily observations for two years are of value, in so far as meteorological appliances are up to the time perfect, and in so far as meteorological readings are comprehensive, the results subjoined may be taken as bona-fide testimonies. They were all collected without any bias, and simply as an inductive means for arriving at a conclusion. I exclude in this chapter, for the purpose of keeping to the point, all mention of any other diseases save those named in my programme, *i. e.*, I keep to the spreading diseases.

Observations, nevertheless, were taken with regard to every disease with which I could become conversant, and the precise date of the commencement of which could be ascertained. As it would be too diffuse a task to give, in general terms, a full history of these two years labours, I have condensed them into a tabular form—so that there may be seen at a glance, the relations existing between the meteorological changes and the special diseases under consideration, the commencement of the diseases, and the periods at which such of them as were marked by fatality, proved fatal.

That the reader who may be disinclined to wade through all these heavy details may be assisted in his task, I append at the end of the tables a summary of results, referring to the tables themselves for the confirmation of my conclusions.

GENERAL BAROMETRIC READINGS AND DISEASE.—

In the following table I have condensed the history of the diseases occurring in the two years referred to, in relation to their occurrence above or below the mean barometric reading. The mean reading of the barometer in my observatory for the ten preceding years was 29.825in.

TABLE I.

| Diseases. | Above. | Below. | Total |
|------------------|--------|--------|----------|
| | Cases. | Cases. | Of Cases |
| Small-pox | 2 | 6 | 8 |
| Measles | 26 | 10 | 36 |
| Scarlatina | 32 | 14 | 46 |
| Diarrhœa..... | 216 | 99 | 315 |
| Dysentery | 6 | 4 | 10 |
| Influenza..... | 63 | 46 | 109 |
| Ague | 5 | 6 | 11 |
| Typhus | 5 | 2 | 7 |
| Erysipelas | 7 | 6 | 13 |
| Catarrh | 19 | 12 | 31 |

GENERAL THERMOMETRIC READINGS AND DISEASE.—In the following table I have traced out the number of diseases commencing at periods above or below the mean temperature of the weeks in which the diseases occurred.

TABLE II.

| Diseases. | Above. | Below. | Total. |
|------------------|--------|--------|--------|
| Small-pox | 2 | 6 | 8 |
| Measles | 12 | 24 | 36 |
| Scarlatina | 20 | 26 | 46 |
| Diarrhœa..... | 158 | 157 | 315 |
| Dysentery | 6 | 4 | 10 |
| Influenza..... | 35 | 74 | 109 |
| Ague | 3 | 8 | 11 |
| Typhus | 4 | 3 | 7 |
| Erysipelas | 6 | 7 | 13 |
| Catarrh | 16 | 15 | 31 |

GENERAL INFLUENCE OF SEASON ON DISEASE.—In the table next subjoined I have indicated the cases of disease occurring in each month and quarter of the two years, so as to show the influence of season.

TABLE III.

| Diseases. | Jan. | Feb. | Mar. | Total 1st Quarter | April | May. | June. | Total 2nd Quarter | July. | Aug. | Sep. | Total 3rd Quarter | Oct. | Nov. | Dec. | Total 4th Quarter | Grand Total. |
|---------------|------|------|------|-------------------------|-------|------|-------|-------------------------|-------|------|------|-------------------------|------|------|------|-------------------------|-----------------|
| Small-pox .. | 1 | 1 | ... | 2 | ... | 3 | 3 | 6 | ... | ... | ... | ... | ... | ... | ... | ... | 8 |
| Measles | 4 | ... | 2 | 6 | 9 | 2 | ... | 11 | ... | ... | ... | ... | 2 | 5 | 12 | 19 | 36 |
| Scarlatina... | ... | ... | 3 | 3 | 12 | 24 | ... | 36 | 5 | ... | ... | 5 | 1 | 1 | ... | 2 | 46 |
| Diarrhoea ... | 11 | 23 | 11 | 45 | 11 | 24 | 11 | 46 | 20 | 37 | 68 | 125 | 60 | 20 | 19 | 99 | 315 |
| Dysentery .. | 4 | 2 | ... | 6 | 2 | ... | ... | 2 | ... | ... | ... | ... | 1 | 1 | ... | 2 | 10 |
| Influenza ... | 20 | 6 | 14 | 40 | 4 | 13 | 10 | 27 | 1 | 3 | 3 | 7 | 9 | 14 | 12 | 35 | 109 |
| Ague..... | 3 | 1 | 1 | 5 | ... | 2 | ... | 2 | ... | ... | ... | ... | ... | 3 | 1 | 4 | 11 |
| Typhus..... | ... | ... | ... | ... | 1 | 1 | ... | 2 | ... | ... | ... | ... | 2 | 3 | ... | 5 | 7 |
| Erysipelas .. | ... | 2 | ... | 2 | 3 | 2 | ... | 5 | 2 | ... | ... | 2 | ... | 3 | 1 | 4 | 13 |
| Catarrh | .7 | 2 | 5 | 14 | 6 | ... | ... | 6 | ... | ... | 1 | 1 | 4 | 2 | 4 | 10 | 31 |

In Table IV. there is given the number of cases of disease occurring during the manifestation of ozone, as contrasted with cases occurring during the absence of ozone.

TABLE IV.

| Disease. | Ozone. | No Oz : | Total. |
|------------------|--------|---------|--------|
| Small-pox | 5 | 3 | 8 |
| Measles | 10 | 26 | 36 |
| Scarlatina | 26 | 20 | 46 |
| Diarrhoea..... | 69 | 246 | 315 |
| Dysentery | 3 | 7 | 10 |
| Influenza..... | 28 | 81 | 109 |
| Ague | 2 | 9 | 11 |
| Typhus | 1 | 6 | 7 |
| Erysipelas | 1 | 12 | 13 |
| Catarrh | 15 | 16 | 31 |

DIRECTIONS OF THE WINDS AND DISEASE.—In the following table there is indicated the occurrence of diseases during the two years, in reference to the direction of the wind.

TABLE V.

| Diseases. | N. | NE. | E. | SE. | S. | SW. | W. | NW. | Total. |
|-----------------|-----|-----|-----|-----|-----|-----|-----|-----|--------|
| Small-pox..... | 1 | ... | .. | 1 | 3 | 2 | 1 | ... | 8 |
| Measles..... | 4 | 12 | 5 | 1 | 6 | 2 | 4 | 2 | 36 |
| Scarlatina..... | 10 | 3 | 7 | 1 | 4 | 1 | 13 | 7 | 46 |
| Diarrhoea..... | 32 | 51 | 20 | 21 | 30 | 54 | 57 | 50 | 315 |
| Dysentery..... | 1 | 1 | 3 | ... | 1 | 3 | 1 | ... | 10 |
| Influenza..... | 21 | 15 | 12 | 3 | 14 | 8 | 22 | 14 | 109 |
| Ague..... | 2 | 3 | ... | ... | 1 | 1 | ... | 4 | 11 |
| Typhus..... | ... | 1 | 3 | ... | ... | 1 | 1 | 1 | 7 |
| Erysipelas..... | 2 | 1 | 1 | ... | 2 | 2 | 5 | ... | 13 |
| Catarrh..... | 4. | 8 | 2 | ... | ... | 3 | 5 | 9 | 31 |

The meteorological elements upon which the foregoing tables have been based, will be found at the end of the work.

CONCLUSIONS.

While the meteorological observations given above are interesting from their general results, it is to be admitted that they throw no direct light upon the origin of the epidemic diseases. The results may be condensed into a few sentences.

In reference to *the barometer*, measles, scarlet-fever, diarrhœa, dysentery, influenza, typhus, erysipelas, and eatarrh, were most common when the barometrical reading was above the mean ; while small-pox and ague were most common when the barometrical reading was below the mean.

As regards *temperature*, the inferences which these labours afford are that diarrhœa, dysentery, typhus, and eatarrh, commenced most frequently when the thermometer stood above the mean temperature ; while small-pox, scarlet fever, measles, influenza, ague, and erysipelas, commenced most commonly when the thermometer was below the mean temperature.

In relation to season, small-pox stood as most prevalent in the second quarter. In this respect my results are different from those of Drs. Tripe and Richardson. These place the disease as most prevalent in the first quarter, and as their figures are massive as compared with mine, I must assume that the difference of my result may depend upon the scantiness of my materials in respect to this one disease.

Measles was most prevalent in the second and fourth quarters. This result is identical with the results of the authors named above.

Scarlet fever in my cases was most prevalent in the second quarter. This also is different from the results of the above-named statistis, who place the disease as most prevalent in the fourth quarter.

Diarrhœa in my table stands out as most prominent in the third quarter. This is in entire accordance with the results of the above-named authors.

Dysentery, of which a very limited number of cases occurred, stands most prominently in the first quarter. There is a difference here between these results and those of my contemporaries, who place the preponderance of this disease with diarrhœa, in the third quarter, and I doubt not with greater correctness.

Influenza was most common in the first and fourth quarters. This is in entire accordance with Dr. Richardson's statistics.

Typhus and erysipelas were most prominent in the fourth quarter,—a result which coincides with those obtained by the last-named author, and in the main as regards typhus, with those of Dr. Tripe.

Catarrh was most prevalent in the first and fourth quarters.

As regards *oxone*, measles, diarrhœa, dysentery, influenza, ague, typhus, and crysipelas, were most

prevalent in the absence of ozone ; whilst small-pox and scarlet-fever were most prevalent when ozone was present.

Scoutetten and many other observers have been of opinion that catarrh is most common during periods when ozone is indicated. It will be seen that in my observations this hypothesis is opposed ; and that there is a preponderance of one case in favour of those periods when no ozone was manifested. This variation is too slight however for any definite inference, except to show perhaps that ozone has little influence either way.

In respect to the *winds*, it will be seen that measles, ague, and catarrh, were most prevalent when northerly winds prevailed ; scarlet-fever, diarrhoea, influenza, and erysipelas, when westerly winds were prevalent. Small-pox was most common with a south wind, dysentery with an east, and south-west wind, and typhus with an east wind.

The only absolute connection that can be traced between diseases of the epidemic class and meteorological influences, is that certain epidemics are specially related to particular seasons. We are led of course to infer from this, that these special seasons are either favourable or adverse to the development of special diseases. The same rule might be applied to the influence of seasons on vegetation in its various forms ; but season alone would never account for the absolute fact of

vegetation. If men observing the transitions of the vegetable world, knew nothing of seeds and germs, they might speculate on the seasons as the producers or destroyers of the plant-world; and with quite as much reason as some of our moderns, who argue for the production of disease by season, through its meteorological influences.

GEOLOGICAL NOTE.

To make this chapter more complete, I append a brief account of the geological condition in which these observations were taken, for which I am indebted to my friend Mr. James Wyatt, F.G.S.

The geology of the county of Bedford is comprised in the mesozoic, or secondary formation, and confined to the Oolitic and Cretaceous systems. Several of the divisions of these two systems, however, are exhibited in a small range of country. For example, six miles to the north west of the county town the great Oolite appears. On the north-eastern side of the county, and extending across to the south-west, are the lower green-sand, the upper green-sand, and the intermediate bands of clunch and gault. Beyond these, to the south, the chalk appears in the ridge locally termed the "Downs," being a portion of the well-known Chiltern hills. The immediate neighbourhood of Bedford, and other considerable portions of the valley of the Ouse, are in the middle division of the

Oolite, particularized as the "Oxford Clay," and they display the ordinary features of that section, soft limestones, and grey, shaly, and dark coloured tenacious clays of great thickness. The general level of the Bedford district is but little broken, and the undulations, where they do occur, are very slight.

Beside the chalk hills at the southern extremity, the only important elevations are some long-stretching ridges of tough clay. One of these extends from Northamptonshire, along the north-eastern side of Bedfordshire, into Huntingdonshire. In some parts this ridge is nearly 100 feet above the ordinary level : this, however, is not due to any feature of the Oolitic formation, nor to any subsequent upheaval ; but is composed entirely of Boulder clay, the deposit of the glacial drift, and altogether different in character to the Oxford clay in the valley. A similar ridge exists on the north-western side of the county, along a portion of the Buckinghamshire boundary. It would appear that this deposit extended over the whole space now known as the Bedford valley, but was subsequently divided by the passage of a freshwater current. It is probable that by some oscillation or change of levels, the drainage of a large area was diverted, and the torrent scoured its own passage through this Boulder clay. The ancient river so formed must, at one period, have flowed to a width of several

miles ; as the gravel deposit extends across the whole of the present valley, and up the flanks of the ridges themselves. The evidence in support of this view is found in the Boulder clay itself, which contains fragments of igneous and other ancient rocks, and the fossils of older and newer sedimentary deposits than the Oolite on which it rests. Further evidence is found in the valley. There, resting upon the Cornbrash, and harder limestone beds, the deposit of gravel referred to—amounting in some places to a depth of 20 feet—is composed of fragments of rocks of several formations, and fossils : in fact, the débris of the Boulder clay. The character of this gravel drift is now very satisfactorily determined. That it was fluviatile is proved by the presence of land and freshwater shells ;—and that it was Post Tertiary there can be no doubt, as the lowest beds of the gravel contain large quantities of the bones of the *Elephas primigenius* and several of its contemporaries. It is worthy of remark that the investigations as to the origin of these large gravel deposits in the valley of the Ouse led to the discovery of the very remarkable, early specimens of man's handicraft, the "Flint implements of the Drift," which have excited so much interest amongst the scientific bodies of Europe. The gravels of the Ouse valley and of the north of France are thus proved to be contemporaneous.

The present Ouse is a very degenerate representative of the ancient river. It flows sluggishly through the wide valley over which the grand old torrent bore absolute sway, but by its wonderful meanderings irrigates a large extent of territory, making it a land of Goshen for fertility, but also keeping perpetually saturated the subsoils and cold clays, and producing a generally humid atmosphere.

The quality of the drinking water used by the population consequently varies within a small circuit. Shallow wells near the Ouse are of course supplied with the river water more or less filtered. Similar wells sunk in the gravel necessarily hold merely rain soakage and fluctuating land-springs; but those which are sunk through the first band of the Oolite rocks are constantly supplied with bright but very hard water. The character of the water in the chalk Downs is similar to that in all other chalk districts; and the Boulder clay yields no water, but may be made to hold rain-fall like a tank.

CHAPTER IX.

OBSERVATIONS ON THE RELATIONS OF VARIOUS
METEOROLOGICAL STATES TO PARTICULAR
DISEASES, CONTINUED.

On meteorological manifestations and their relations to disease, derived from forty-five years of daily recorded observations by H. W. Bailey, Esq., of Thetford. Results of medico-meteorological observations in several parts of the country and in London, as deduced by Messrs. Ransome and Vernon.

In the present short chapter I include a condensed history of the results of forty-five years daily observation of meteorological states and their bearings on disease, by my friend H. W. Bailey, Esq., of Thetford, Norfolk. So faithfully have these records been kept, and arranged with such simple earnestness and originality, that I cannot but deeply regret the entire impossibility of giving them in all their fulness. The succeeding abstract from the hand of my learned friend himself, will nevertheless repay perusal.

“In spring, various forms of angina, croup, bronchitis, inflammations, catarrhs, rheumatism, etc., and an occasional case of fever. In summer, fevers running into the typhoid type, diarrhœa,

erysipelas, and choleraic diarrhœa. In the autumn, these diseases sometimes become more prevalent, especially choleraic diarrhœa, in some instances running into dysentery; ague, remittent, cynanche tonsillaris, enteritis, and some inflammatory affections. In the winter, influenza, rheumatism, inflammations of the thoracic viscera, low typhoid fever amongst the poor. Referring to my meteorological register, I find that the weather has considerable influence on the prevalence of the more common diseases. Droughts are unfavourable to pulmonary diseases; and during very wet seasons, remittents and intermittents, catarrhal and rheumatic affections, are prevalent. High and very cold winds occasion inflammatory affections of the lungs, rheumatitis. Warm and hot winds occasion bilious diseases, diarrhœa, and ophthalmia. Calm and humid states of the atmosphere produce continued fevers, and extend infectious and contagious maladies. Epidemic diseases are much influenced by locality. In those places in which the soil is light and sandy, the evaporation rapid, and the prevailing winds from the W. and NW. and SW., the air being freer from moisture, health prevails, and only those diseases come under notice which usually occur at particular seasons. I therefore cannot but suppose that the above diseases must depend upon some other causes acting upon the human system generally.

As regards endemic diseases, these depend mostly upon peculiarity of locality, independently of temperature,—the nature of the soil, the elevation above the level of the sea, the connection with woods and water, and also valleys, etc. That low and swampy grounds give rise to intermittents is well known, the air being supersaturated with moisture, and probably impregnated by certain effluvia from the vegetable productions. In this neighbourhood, the soil being light and sandy, the rains are soon absorbed and but little evaporation takes place, so that very few endemic diseases ever come under the notice of medical men. That particular diseases should arise in certain districts, much depends on some morbid poison generated from the soil or present in the water, and either imbibed or taken into the system through the circulation, and producing those diseases incident to the identity of the poison,—in the same way as the forms of small-pox, measles, etc., producing their like diseases. These diseases, although running through many families, cannot be said to be contagious in the strict sense of the word, and cannot be communicated to others who are strangers to the locality, and who accidentally visit those afflicted with the disease. The same may be said of the common epidemic diseases, and if communicated, will be found to attack those who are exposed to the same deprivations, habits, and bad lodgings.

They seldom or ever attack those in the higher walks of life, a fact which I have witnessed numerous times in my professional observations."

There can be little doubt that a continued series of faithful observations, showing the precise dates of the commencement of several diseases, side by side with precise records of meteorological changes in the localities of the diseases, would be useful in settling many doubtful points in pathology. In 1853, I was mainly instrumental in organizing a plan of recording atmospheric changes and the consentaneous origin of diseases in several localities. The results were recorded in the Association Medical Journal. The plan was fully developed in an article in that Journal of August 26, 1853, entitled "Medical Meteorology;" and observations were regularly supplied for a period of two years and a quarter from the following places—Guernsey, Ryde, Exeter, Uckfield, Bedford, Grantham, Hawarden, and Wakefield. Unfortunately these records were not continued for a longer period, in consequence of the little interest then taken in these investigations, and of the space occupied by the weekly reports being begrudged by many of the readers of the Journal.

In 1857, the General Board of Health in London took up the subject; and from the week ending April 11, 1857, to the week ending November 6, 1858, it published a carefully compiled weekly

return of new causes of disease in London, furnished by the voluntary efforts of upwards of two hundred gentlemen connected with the medical profession. The tables are accompanied by meteorological observations made at six stations in and out of London, and it is very much to be regretted that these valuable observations also were soon discontinued. The results of these two series of observations have been carefully brought out by Messrs. Ransome and Vernon,* and they have deduced some general conclusions which I have thought desirable to embody in the present chapter, as bearing upon my own researches in this department.

The method employed by Messrs. Ransome and Vernon in making the necessary comparisons of the two series of observations, and the conclusions at which they arrived, are given in their own language in the following sentences:—

“We have projected the medical and meteorological returns upon separate charts, so as to form curves, which represent the prevalence of the disease or the state of the atmosphere at any particular time; and then, by comparing the two charts, and noting any evident coincidences, we have been led to the conclusions specified in the paper,

* On the Influence of Atmospheric Changes upon Disease: by Arthur Ransome, M.B., B.A., and George V. Vernon, F.R.A.S.; reprinted from the first volume of the third series of *Memoirs of the Literary and Philosophical Society of Manchester*.

respecting the following diseases—diarrhœa, dysentery, pneumonia, bronchitis and catarrh, pleurisy, continued fever, rheumatic fever, measles, whooping-cough, and scarlatina.

DIARRHŒA.

A high mean temperature (above 60° F.) would seem to have a powerful influence in predisposing to this disease ; when continuous causing a rapid increase in the number of cases.

A temperature below 60° F. appears to be unfavourable to its progress.

DYSENTERY

(1). Seems to be influenced by the variations in the mean temperature, but in less degree than diarrhœa, the effect not being generally traced in the lesser undulations of the curve.

(2). Increased atmospheric pressure seems to be unfavourable to the progress of the disease, high readings of the barometer being nearly always accompanied by diminished prevalence of dysentery.

PNEUMONIA

Seems to be very greatly influenced by the mean temperature, the disease curve rising as the temperature falls, and *vice versâ*.

BRONCHITIS AND CATARRH.

The curve of these diseases, although drawn from ten times the number of cases, is almost identical with that of pneumonia, its highest and lowest points coinciding exactly with those of the pneumonia curve.

PLEURISY.

This disease is too irregular in its course to yield any information in the present investigation, as the meteorological elements under consideration do not appear to have any apparent connection with it.*

CONTINUED FEVER.

It is difficult to trace any connection between the progress of this disease and the meteorological elements under consideration; but on the whole, high temperatures seem rather favourable to its production, and extreme cold is probably opposed thereto.

RHEUMATIC FEVER.

The curve of this disease is not sufficiently extended to admit of accurate comparison with the meteorological curves, and therefore no decided conclusion can be drawn respecting it.

Our data, however, would bear out the observation of Sydenham,—‘This disease may come on at any time; it is commonest, however, during the autumn.’—Obs. Med. vi., 5 (1).

MEASLES.

In its chief undulations the measles curve seems to rise with the fall of the temperature, and *vice versa*; and the influence of this element is best marked when it is above or below the forty-three years’ average.

* Among the seasons of the year, winter more especially engenders the disease, next autumn, spring less frequently, but summer most rarely.—Arctæus, *Causes and Symptoms of Acute Diseases*, book i, chap. x.

WHOOPIING-COUGH

Seems to be much influenced by the extremes of heat and cold, the curve, on the whole, rising with the fall and sinking with the rise of temperature.

SCARLATINA.

A large amount of aqueous vapour in the air appears greatly to facilitate the formation and action of the peculiar scarlatinal poison, especially when this is accompanied by sudden fluctuations in the atmospheric pressure as shown by the barometer; a diminished pressure being favourable to the disease.

It is rather difficult to separate the influence of temperature from that of humidity, but a moderately low temperature seems to be favourable to the progress of the disease, whilst the extremes of both heat and cold seem often to exert a disturbing influence one way or the other; a temperature above the average generally diminishing, cold increasing the number of cases."

The propositions respecting diarrhœa, dysentery, pneumonia, bronchitis and catarrh, and measles, receive further support from a subsequent analysis year of diseases occurring in Manchester during the 1861. The conclusion with regard to whooping-cough is not borne out by that analysis; and scarlatina was not prevalent in Manchester and Salford during the period referred to.*

* Analysis of the weekly returns of the Health and Meteorology of Manchester, from September 8, 1860, to December 28, 1861. By Arthur Ransome, M.B., M.A., and George V. Vernon, F.R.A.S. Manchester, 1862.

CHAPTER X.

ILLUSTRATIONS OF THE ORIGIN AND PROPAGATION OF
DISEASES BY INFECTED AIR.

*Cases of Typhus and Typhoid Fevers. Of Cholera. Of Remittent Fever.
Of Scarlet Fever. Of Erysipelas. Of Small-pox. Of Puerperal Fever.*

In the present chapter it is my intention to place before the reader a series of cases illustrating the possible and, in many instances, the probable origin and propagation of diseases from malaria, that is to say, from infected air. The cases given will include some from my own practice, but a majority, by far the greatest, from the practice of medical friends, who supplied the facts in reply to a request issued by me, in a manner I shall not readily forget.

Suffice it to say in the way of introduction to the examples of disease named, that they were all well marked types of the families to which they respectively belonged, and that the circumstances under which they occurred were observed in the most careful and trustworthy manner. However much, therefore, opinions respecting the origin or propagation of the diseases may differ, the *facts* must be

accepted ; and on the facts, apart from the opinions, I base all the value that attaches to the histories appended.

1. CASES OF TYPHUS AND TYPHOID FEVERS.

Observation 1. A gentleman under my own care had often complained of the offensive smell in the lower part of his house on going down in the morning ; and was compelled to open the windows and doors for some time, in order to remove it. This arose from a neighbouring drain, the emanations from which had frequently been so copious as to tarnish the candlesticks in the kitchen, and the brass handles of all the doors,—not only of his own house, but of the adjoining house. In August, 1857, this gentleman was seized with fever, which he had so severely as to be confined to his bed for four weeks. During his illness a nephew from another part of the country visited the next door, but had no intercourse with his uncle. The nephew returned home and was directly afterwards seized with fever. After the gentleman's recovery, a shop-girl was also seized with fever, which went through precisely the same course. In this instance the water-supply was good, and removed from any source of contamination.

Observation 2. In the immediate neighbourhood of a slaughter-yard in St. Paul's square, Bedford,

the drainage is very deficient, fever has been rife for some years past. The exhalations are so powerful as to have been known frequently to have discoloured plated brass, and silver articles hanging up inside the houses. In one row of houses, in the autumn of 1849, a tradesman's wife and daughter, whom I attended, were attacked with fever, and the wife died. The members of another family in the adjoining house were all, except the husband, seized with fever in 1850. This fever was so virulent that it was with difficulty the affected family could procure a nurse to attend them. In another house in the same row several members of one family were seized, and two died. In 1853, several members of a fourth family were seized. On this occasion the fever continued in the house many months, and one person died from it. In 1853 and 1854, a large family in the same locality was attacked with fever. Two of the children suffered from it severely; one of them, a girl of twelve years of age, was confined to her bed for four months. They both recovered.

The fever in all these cases was pure typhus, and many of the sufferers from it, after partial or complete recovery, were subjected to relapses. The origin of the disease seemed wrapped up entirely in local causes. I attended strictly to hygienic improvements, in the end secured effective drainage and ventilation, and the disease has not recurred.

In these last-named cases there is a difficulty in specially referring the cause of the phenomenon to atmospheric impurity. For the water during the prevalence of the epidemic was equally impure. The new improvements have led also to an improved water for domestic purposes.

Observation 3. Dr. Frederiek J. Brown of Chatham, has communicated to me the following case. "In the city of Rochester there was a house adjoining a privy-cesspool which had long been closed. One day this cesspool was opened; typhoid fever of a malignant type at once showed itself in the house, and three children died from it. The connection of cause and effect in this instance was immediate and unmistakable."

Observation 4. The same practitioner has given me the three following histories. "At a village near the same place was a small court entirely undrained; fæcal matters accumulated in it, and typhoid fever and scarlet fever were almost always present. At last the place was barrel-drained; the people remain in the same circumstances as formerly, and are personally as dirty as before, yet the diseases referred to have entirely disappeared."

Observation 5. "There was a house near a burial-ground in my town. The families residing

in these houses continued one after another to suffer from typhoid fever. Not content with the idea of the burial-ground as the cause, but believing in the privy-soil-emanation origin of typhoid fever, I inquired carefully into the cause of the disease. At last I discovered a cesspool underneath the kitchen of this house. The cause was removed and the disease has not reappeared."

Observation 6. "I was one day passing the grating of a foul sewer; the smell from it struck me powerfully, communicating a peculiar sensation to the throat. I went along conscious that I had inhaled a poison. On the following day I became ill, and suffered so severely for ten days from diphtheritis, that I was confined to my bed and recovered slowly."

Observation 7. In a yard in the town of Bedford, the drainage of which is most deficient and offensive, —every inhabitant bearing unequivocal testimony to the occasional almost insupportable nature of the stench—scarcely a house was free from fever during the summer and autumn of 1857, and some of the cases were of an obstinately relapsing character. So offensive was the smell external to the houses, that the windows were purposely closed in order to avoid an aggravation of the mischief. The water-supply was also deficient.

Observation 8. In a spacious house in Bedford, kept exceedingly clean, and by no means overcrowded, fever showed itself in a lad during the summer of 1857. He escaped narrowly with his life, and was convalescent, when he had a relapse, and recovered with the greatest difficulty. The freest ventilation was kept up day and night, and every precaution was taken to isolate him from the other members of his family. Some time afterwards a servant was seized—went home and died. Afterwards a younger sister of the youth was seized, and struggled through with difficulty. Next the father became ill with fever and died. A slaughter-house existed at the back of this house, but was kept in the utmost state of cleanliness; at the back of the premises, however, was a large cesspool and a very offensive drain. The water-supply in this instance was good. The well was removed from the cesspool, and the water itself gave no evidence of organic contamination.

Observation 9. The two succeeding cases were communicated by Mr. W. J. Cox of Hawkesbury. “In January and February 1849, I had the temporary charge of the sick poor of the district of the Poplar Union, including the town of Bow and the adjacent village of Bromley. A part of the village consisted of a long, squalid, double row of houses in an unfinished state, no proper draining having

been made. The street itself was nearly choked up with refuse and ordure. The inhabitants were mostly Irish, and dwelt in rooms fearfully overcrowded. The effluvia in their rooms were intolerable. During the period of my stay I had thirty-eight cases of typhus in this one street. In every house save one lay a sufferer from the fever at the same time, and in many of the dwellings there were two sufferers. In the third week of January twenty-two lay ill, and five died, and I learned that fever was constantly present. At that period typhus was not epidemic in the environs ; and but one case occurred during my stay, out of the pestilent street, and that in a young woman who had come from a healthy country district to see her parents, who immediately before her arrival, had spent two days with a married sister living in the infected locality.”

Observation 10. “In January 1856, I was called to attend a case of low continued fever in a detached and apparently healthily situated cottage in the outskirts of the village of Abram in Lancashire. The Incumbent of the district who had requested my attention to the case, remarked to me as we went together to the house ‘ you scientific men are at fault here. You would be puzzled, I fancy, in this case to trace the malady to overcrowding, dirt, or profligacy. I know the people of the house well, they are very poor, but cleanly in their persons and habits, sober

and well-conducted. Nevertheless, there have been three fever cases in the house within the last two years, whereof one terminated fatally.' I replied, 'perhaps you are not in possession of all the circumstances of the case.' This turned out to be a correct surmise. On my arrival I found indeed the cottage clean, but there was no drainage! The privy opened into a large cesspool which was scarcely ever emptied. The cesspool was then overflowing and saturating the ground round about. The poor people told me that the smell throughout the house was frequently overpowering, especially at the approach of rain, and during sudden changes in the atmosphere. They said the privy almost always threw off the odour of 'rotten eggs,' and often affected them with nausea on first entering it. It is necessary to mention that at the time I speak of there were no fever cases in the neighbourhood, and no epidemics of any kind, either in the adjacent village or in the neighbouring town. The fever evidently originated on the premises, and fortunately, from their isolated position, it was confined to that locality.

Observation 11. A school-establishment in the town of Bedford had been broken up on three occasions within five years, in consequence of the appearance of fever among the scholars. It was found that a cesspool at the back of the school had

become full and very offensive. This was filled up, and fever has not since been known. A neighbouring house was scarcely habitable at times, in consequence of the emanations from this cesspool, and the family constantly suffered from fever until the cesspool was removed.

Observation 12. Dr. Septimus Lowe of Lincoln, obliged me with the following observation. "I had lately under my care a family in the town of Lincoln, ten of whom suffered from continued fever; one of the patients died after an illness of twelve days. I traced the fever as originating from offensive emanations arising from two cellars beneath the house, which had been for several weeks during the hot weather covered with at least two feet of water, and evolving most offensive odours. The cellars were drained, and the remaining members of the family recovered favourably from their attacks. A relative who came to attend them was also seized with the same fever. There has scarcely been any illness in the family since."

Observation 13. Dr. M^cIntyre of Odiham, sends me the brief but telling description of the sanitary condition of his town. "There are two parts of this place, one undrained and containing numerous cesspools, and the other well drained. Some years ago a smart epidemic of typhoid fever, proving unusually fatal, killing one in five, occurred in this

town, and was almost entirely localized in the undrained part. In one house alone in this place several cases occurred of uncommon severity. In this house a drain ran under the bricks of the floor, and charged the house with its emanations."

Dr. M^cIntyre also remarked that in the same part—the lower part—of the town, where fever at that time raged so fiercely, out of three severe surgical operations, one of encysted tumor of the face, one of excision of the shoulder-joint, and the other of inguinal hernia, the two former were attacked with erysipelas without apparent cause. The tumor case nearly perished from profuse suppuration under the scalp, and the shoulder case, a man aged 62, did prove fatal.

Observation 14. Typhus appeared in two cottages adjoining each other in the village of Wilshamstead, four cases in one and one in the other. There were dung-pits close to the doors, and case after case occurred until these dung-pits were removed, and cleanliness enjoined. After this removal the disease disappeared altogether and has not since returned. The water-supply was good and remains unaltered.

Observation 15. The next five observations are communicated by Mr. Eddowes of Pontesbury. "In the latter end of August 1856, we had continuous rain for three days ; about a week afterwards as I rode through a neighbouring village, I smelt a

most offensive odour. On the 18th of September, typhus and typhoid fever with diarrhœa broke out simultaneously in several cottages of the village. It attacked the inmates of nine dwellings ; one child died and one woman. One death occurred in seven cases. The woman lived in a miserable house in a low and damp situation, with but little light and ventilation. The other dwellings were well situated. The cause of the smell, and no doubt the origin of the fever, was the stoppage of a sewer which led from seven cottages. The village contains twenty dwelling houses."

Observation 16. "On the 11th of October 1856, I was called to see a case of typhus. The case occurred at a house adjoining a mill. The house was quite isolated. Two sisters and a brother suffered from the disease but recovered. From the general sanitary arrangements I was at first in a difficulty to account for the origin of the disease. At length I found beneath the floor of the house the cesspool of a privy which had no outlet, and was only separated from a pantry in the house by a very thin partition. The emanations from this pantry were intolerable, and I could trace no other origin for the malady. The disease did not spread."

Observation 17. "At the time I was attending the typhus cases noted above, I had other cases of

typhus several miles distant from those. I could never distinctly trace the origin of these cases. The first case occurred at the house of a carrier who attended a neighbouring market once a week, and had to pass through a village where typhus was present. The carrier's wife first succumbed to the disease. She was taken ill on the 20th of August, just one month before the fever appeared in the village through which he had to pass to market. The husband was taken ill with decided fever on the 24th of September. The wife's case I thought at the time was a doubtful case of fever, as it consisted principally of obstinate bilious attacks; but in the husband's case the disease was definite. From his house it was carried, by a woman who waited on him, to a hill-side at a considerable distance. Here it spread; the habitations on the hill were open on all sides, but about twenty suffered from the disorder, and the locality remained infected for full three months, and one patient, a boy, died. In one miserably small house built of turf, and dark and damp, having only two small windows, and but one sleeping apartment on the ground floor, there were several sufferers."

Observation 18. "Typhus one day broke out suddenly in two cottages three miles from my own residence; the cottages were well situated, and there were no similar cases in the neighbourhood.

Another practitioner was in attendance, and I was called in, in consultation. We were at a loss at first to account for the disease, but on inquiry, I found beneath the stairs of one of the houses a heap of diseased and decomposing potatoes, from which an offensive emanation arose."

Observation 19. "In a farm-house in a village two miles from my residence typhus broke out. The external sanitary arrangements were good, but in the cellar of the house was a large store of rotten potatoes, from which there proceeded a most offensive smell. The people were habitually dirty, and two years before they suffered from fever, from what I believe to have been the very same cause. The fever was confined on both occasions to the one house."

Observation 20. I am indebted to Dr. Maekinder of Gainsborough, for the subjoined facts. "The cases I have to describe suddenly appeared, they were several in number and four proved fatal; they were all cases of fever. They arose in a particular spot, and within a circle described in a few hundred yards, in the centre of which was an unused well, which had been for some time the receptacle of dead animals, and from which a most offensive effluvium emanated, and extended for a considerable distance. Attributing the disease to this cause, I

had the well covered in ; the disease disappeared as if by a charm, and there has not been a case of fever in that locality since."

Observation 21. The annexed case was communicated by Mr. Rumsey of Cheltenham. "I have good reason to recollect," says he, "a singular invasion of fever in my own family. My wife, four children, and a nurse were at the seaside in 1845, at Bude, on the coast of Cornwall, a fine open sea coast. A little stream however, opened into the sea in the midst of the village, and this stream was used as the main sewer of the place. The summer was hot, and the bed of the little river partly dry, and there was at hand some decomposing seaweed. Three or four other families in the place were attacked, and several cases ended fatally. They carried the fever with them to their respective homes ; mine did so, and though the children were spared, our nurse died, after communicating the fever to another servant in Gloucester, who also died. I caught the fever in a milder degree and soon recovered. Several villagers at Bude, on the coast, were attacked in a similar way."

Observation 22. The village of Cople near Bedford, had been notorious for typhus fever, and the death-register shows a considerable mortality from

fever in that village for many years past. It is low-lying, and by the side of the street, along a considerable portion of the village, was a large wide open ditch, which in rainy seasons conveyed the water from the neighbouring hills to the brooks. This ditch in dry seasons was partly filled with stagnant water in many places, in other places was merely covered with mud and decomposing vegetable matters; it was also the receptacle for privy-matter and every kind of filth. It was considered that this ditch had something to do with the frequency of fever in the village, and on representation being made to the proprietor of the village, the late Duke of Bedford, a deep water-course was carried through the fields at a distance from the cottages, and the old shallow ditch was filled in and converted into garden-ground for the cottagers. Not a single case of fever has since been known in this village. The change has been effected ten or eleven years, and is so marked as to have excited the notice of every inhabitant. The water-supply remains in the same condition as it did when the fever was prevalent. In 1832, in this village, cholera presented itself in its most fatal forms. In 1854, not an inhabitant suffered from the prevailing epidemic.

Observation 23. My friend Dr. R. Uvedale West of Alford, sent me an illustration which, as showing recurrence of disease from a cause, is worthy of note.

He writes thus: "In a sea-side village called Mumby-Chapel, I once remarked that typhus was now and then obstinately persistent in a dry summer, and afterwards disappeared. This occurred several times. I traced the disease at last, as it seemed to me very clearly, to be the result of a system of drainage adopted by the authorities. During the dry weather, in order that the cattle might not lose their supply of water, they stopped up the water in the drains, thus rendering them for the time shallow stagnant pools, charged with decomposing vegetable matter, and giving rise to offensive emanations. It was during these times that the fever appeared and was unusually persistent. When the rains came on and the drains again became flowing streams, the fever as regularly disappeared."

Observation 24. During the progress of these sheets through the press, Dr. West also favoured me with the following valuable observation:—"Several years ago, when this town was being supplied with new drains—'barrel tunnels'—along the main streets, I remarked, as the work was proceeding, that at intervals the workmen were making deep cesspools to be provided with gratings above; these being intended to intercept the sediment, lest the latter should, by accumulating in the drains, block them up. In spite of my repeated remonstrances, the work was proceeded with and

completed on the original plan. A few years afterwards, an unusually malignant and fatal form of typhus fever broke out in the town, and created by its persistence so much dismay, that a public meeting was held to devise a plan for the safety of the inhabitants. I suggested that the probable cause of the epidemic was the emanations, through the gratings, of the decomposing sediment in those unnecessary cesspools, and proposed that they should be filled up to the level of the bottom of the tunnels. This was done ; the epidemic disappeared at once, there being no more deaths from that epidemic of typhus, and the dreaded accumulation of sediment which was to block up the drains never took place.”

Observation 25. The history given below of cases of typhoid fever and scarlet fever was communicated by Dr. Moffat of Hawarden. “About the middle of August 1848, there occurred a case of scarlet fever in a village about five miles from Hawarden ; the case proved fatal. This in about a fortnight was followed by other cases of scarlet fever which very rapidly assumed a malignant type. ‘Scarlet fever without eruption’ next showed itself among children of advanced age, and adults ; and by the middle of October, people of all ages were attacked with fever of a typhoid type. By the first week in December, low typhus had assumed

such a serious import, that I called the attention of the local authorities to the state of the parish. The village is situated in a flat level of many thousand acres in extent. To the south and south-west there are extensive meadows, which for the greater part of the winter are more or less 'flooded,' and in summer and autumn are often under water, drained off from the hills to the SW, W, and NW. On the north side there are also collections of water. The village itself is a cesspool of putrefying animal and vegetable offal. At the time of the epidemic the population of the village was 200, and in the course of five months there were in it 68 cases of illness, 61 of which were from fever. I may here mention that the fever gradually spread from this centre all over the parish, and gradually wore itself out in the thinly peopled districts. Believing that the inhabitants of this place were poisoned by the exhalations from the stagnant pools and accumulations of filth in the village, I reported the fact to the Board of Guardians. Sanitary measures were adopted; the village was drained and thoroughly cleansed. The fever soon after began to abate. I believe I may say that within a month or six weeks from the cleansing of the village it was clear of fever, and eight years have now passed without the appearance of a single case. Without taking into consideration the increase of population, I give you the average of deaths for eight years since

sanitary means were adopted. The annual average of deaths from 1841 to 1848 inclusive, before draining, was 23 per thousand. The annual average from 1849 to 1856 inclusive, after draining, was 15 per thousand. Thus in twenty years 32 lives are saved in the above village by attending to a few hygienic rules. To show the effects of rain upon the above low-lying locality, I may mention that the number of deaths in this place was doubled in 1851 as compared with 1852. In 1852, from the 1st of June to 30th of November, 26.8 inches of rain fell, an amount greater by 3.2 inches than the annual average. This amount of rain of course increased the quantity of stagnant water in the village and around it, which in its turn gave off effluvia and poisoned the people. In 1852, a similar epidemic to that of 1848 prevailed in another village about three miles away, and under very similar circumstances. I reported it, sanitary means were put in force and with similar happy results."

Observation 26. Some cases of fever communicated by Dr. Wm. Walker of Islington are given as below. "The locality in which the cases occurred was formerly an open field, grazing pasture-ground for cows ; at that time it was intersected with foul, deep, and stagnant ditches.

The ground was at last laid out for building purposes, the ditches were closed and part was built over. Drainage was but imperfectly introduced, and deep cesspools were dug out in abundance. The houses built were airy, there being ample space before and behind; but at a small distance from them was a stagnant dirty pool, which frequently gave forth offensive smells. The houses were badly built as regards site, some being placed directly over the spots which had originally been ditches. The persons who occupied these houses were all living in comparative comfort, being respectable tradespeople, cleanly in their habits and well fed. These houses had not long been inhabited before typhus broke out in them. I recall two fatal cases, both of which seemed to me obviously to have arisen from the diffusion of an impure air. The first case occurred in a man aged 34, whose house was directly connected with a large cesspool. This man died. The second fatal case occurred in one of the houses which had been built over one of the former ditches. The offensive emanations in this dwelling were such that it was intolerable as a residence, the occupants feeling it necessary to leave ultimately from the mere nuisance. In this house typhus broke out, and attacked with more or less severity every one in it, father, mother, and children. There was one young woman aged 18 years, who had the disease in its severest forms and died."

Observation 27. A gentleman in Bedford laboured under low fever for some time, and on seeking for a local cause, it was found that the water was impure and the drainage in a very ineffective state. Many pailfuls of black filthy matter were taken from the bottom of the well, and a great uncemented cesspool was found within a few feet from it. A child of this gentleman was seized with scarlet-fever, and sank within 24 hours after the commencement of the attack. There was no return of fever after the removal of the causes named.

Observation 28. I am indebted to my friend Mr. Howard Barrett for the following interesting facts. In a neighbourhood of London remarkable for its healthiness, and resorted to in consequence of its good reputation in this respect, is a collegiate establishment which had been erected apparently with every regard to sanitary arrangement. The ventilation was perfect, and the house was by no means crowded. Fever, however, of a low typhoid character made its appearance from time to time, proving fatal in some cases, and in others being very severe with protracted convalescence. At length an old bricked cesspool was found, of large size and quite full, communicating with a coal-cellar under the dining-room of one of the tutors. This was emptied of its contents and then filled up. Since this there have been no cases of fever. The water-supply was from the water-works.

Observation 29. Two deaths had occurred in a house in the town of Bedford from typhoid fever. A third case of the same disease in a severe form occurred some time afterwards. The house had always been subjected at certain times to very offensive odours, and they always seemed to arise from a certain spot. My attention was directed to these bad smells. Certain directions were given for the purpose of finding out their cause. A direct but very insidious communication was found with an old and most offensive cesspool. The local origin in the house of these offensive smells was most satisfactorily discovered, and as satisfactorily removed. The disease has not returned.

Observation 30. In the centre of the town of Bedford, the well water became discoloured and unfit for drinking purposes. A case of fever had occurred in the resident family. The well was ordered to be opened, and it was discovered to have been contaminated by percolation from a manure heap in the neighbourhood. The dung-pit was cemented, and roofed over; and the evil was thus removed.

Observation 31. Some years ago, fever of a typhoid character made its appearance in one row of cottages in the village of Cotton End, affecting every family in that row. The sanitary condition

of the cottages was deplorable. They had been built directly upon the soil, and were not secure against the weather. There was literally no privy accommodation whatever, and the details of the habits of the people living in these cottages would be too disgusting for publication. Suffice it to say, that here was the nidus for fever whenever other circumstances favourable for its development should occur. On the occasion referred to fever took its origin in this row, and afterwards spread by contagion to other parts of the village. Measures of improvement of a temporary character were resorted to by the authorities, and after a time the disease subsided. Some years afterwards, the improvements having lapsed, fever again made its first appearance in this row, with similar results.

Observation 32. The following illustration, occurring in the person of a well-known member of our profession, Dr. Swayne of Bristol, and reported to me by himself, is perhaps one of the best authenticated cases on record. I therefore give it entire. "In the night of November 27th, 1852, I was called up to attend a labour in a neighbouring street. For three months previously I had been out of health, and had suffered much from weakness and irritability of the eyes. I arrived at the house of my patient about two in the morning. As I entered, I met some nightmen, who were

engaged in emptying a cesspool connected with the drains in the house, which had not been disturbed for thirty years. The stench was intolerable. I immediately experienced feelings of nausea and headache, with some prostration of strength. However, I attended my patient upstairs in her confinement. All the doors communicating with the lower part of the house had been kept closely shut, and chlorine had been used freely in the staircase. The result was that no harm came to my patient, who recovered well. It was not so with myself. From that time I began to lose my appetite, to sleep badly, to be feverish at night, and also to suffer occasionally from headache. This went on until December 15th, 1852, when I was attacked with typhoid. At first I suffered much from headache, which was sometimes very intense. There were no well marked gastro-enteric symptoms. The headache was much relieved by leeching and cold lotions, and subsided after a few days. The fever, however, continued without much abatement for nearly a month, after which I began to mend, and in less than six weeks was able to go out a little. The quinine treatment of fever was at that time much in vogue, and was adopted in my case. It did not, however, appear to cut short the fever in any way, although my medical attendants considered that it rendered my subsequent convalescence more speedy.”

Observation 33. Another illustration, very striking from its isolation from sources of fallacy, is supplied me by my friend Mr. W. R. Milner of Wakefield. "Some years ago," writes Mr. Milner, "I attended a succession of fever cases in a row of cottages built quite on the top of a conical hill, so that the ground fell from them in every direction, and there were no buildings within several hundred yards of them. The cottages either belonged to, or were in the occupation of, the overseers of the poor of that township, and were used as residences for pauper families. I learned that they were scarcely ever free from fever. The inhabitants assured me that there were no collections of filth or other nuisances about them. One day, when I was riding at some distance from them, and looking towards the back of the cottages, my attention was arrested by a broad dark line running down the side of the hill. I found on examination that this arose from the coarse rank grass growing on the side of a channel, down which ran the overflowings of a long shallow pond, close to the backs of the cottages, and overlooked by the back windows. The pond was filled with the accumulated filth of years, and was so covered with a mass of green confervæ, oscillatoriæ, etc., that it was easily overlooked, and it was not at all visible from the road leading to the cottages. The evil arising from it being pointed out to the overseers, the pond was cleared out and

filled up. I remained between two and three years in that neighbourhood; and, during that time, no fresh cases of fever occurred in these cottages."

Observation 34. The following cases were also supplied by Mr. Milner. "In the early part of 1849, I visited a number of prisons for the purpose of selecting convicts to be removed to Wakefield. Among the prisons so visited was Lincoln Castle. I there found thirteen men under sentence of transportation, twelve of them were then either suffering from fever, or were convalescent from attacks of fever. On inquiry it appeared that a block of cells constructed on the Pentonville plan had been built, and prisoners had been placed in them, I think for the first time, in October 1848. By the end of the year almost every prisoner who had been placed in these cells was ill, and the greater part had continued fever; at the time of my visit on the 5th of June 1849, the prisoners had all been removed from these cells.

I examined the cells, and found them well built, dry, and provided with good arrangements for warmth and ventilation; they were also perfectly clean, and evidently had been kept so when occupied. They were all provided with water-closets, which were clean and in good order. I then inquired about the drains, and the mystery was at once solved. There were no drains from the prison, but

all the sewage from these cells was conducted into a closed cesspool, which I was most carefully assured had been made *quite air-tight*. The result of this arrangement was that when the tension of the disengaged gases in this air-tight cesspool became greater than the pressure of the water in the syphon trap of the water-closets, large quantities of these gases escaped into the cells, as one of the prisoners told me, 'with a noise like thunder, and a stench that would poison the devil.' "

Observation 35. In addition to observations 15, 16, 17, 18, and 19, Mr. Eddowes of Pontesbury, has forwarded to me the following facts. They are valuable inasmuch as they illustrate very distinctly the conveyance of contagious matter by the person. "During the summer of 1859, a young man left a mercer's shop in Shrewsbury ill. His father is a national schoolmaster, residing in a village sloping towards the south-east, high and dry, and on the red sandstone. It is a small and clean village. Fever was never known there before. A farmer's widow died there about that time aged 101 years. A farmer died there a few years ago at the age of 105 years. When the young man had been at home about nine days it was discovered that he had fever. The children who attended the school soon carried the disease around the neighbourhood. It was brought here by a servant who lived at one of the

farm-houses in the village. The village was infected from one end to the other. The clergyman's sister and servants took it. I was their medical attendant. The village being about five miles from this, I slept at the rector's house, and used to return home early every morning. Being in a weak state of health from over-work for many months, I took fever, with tympanitis of the most distressing kind: but I am now quite well. I was confined to the house six months, to my bed five months."

Observation 36. I shall conclude these notices of fever by recording a case communicated by Dr. Spencer Thomson of Burton-on-Trent. "The following facts," says my friend, "respecting the incubation of typhus fever, may be valuable from the peculiar circumstances of the case, and from my being able to state every particular with well marked exactness. In the year 1847 I took my wife and eldest child to Edinburgh on a visit. With us there went as nurse a country girl from this neighbourhood. During our stay in Edinburgh, and only two or three days before we left, the nurse received permission to go into town with one of my mother's servants, an Edinburgh girl. They walked about a good deal, got fatigued, it being the month of June, and went to have tea with the Edinburgh girl's friends, who lived in one of the old town wynds or closes. After our return to England our nurse-girl never

seemed well, complained of constant dull headache, had a dusky look about the skin, and was in a state of depression both mentally and physically. It was, however, within a day or two of completing the six weeks from the date of our leaving Edinburgh before actual feverish symptoms, compelling retirement to bed, were developed. The girl had a most severe and dangerous attack of typhus fever, which lasted, from the time of her taking to bed till her convalescence, full thirteen weeks, that is to say, till she was well enough to be removed from my house. I had been accustomed to see much of the Edinburgh fever during my studentship. I had been away from Edinburgh for ten years, and I have often remarked that I had not in the ten years seen a case of the old Edinburgh typhus fever, till it occurred in my own house in the person of this nurse-girl who brought it, I have not the slightest doubt, from Edinburgh with her. She stated afterwards that when she went into the house in the old town where she took tea, she perceived a sickening smell which she never got rid of, and she felt ill ever afterwards. I sent my wife and child from home, and kept the case isolated, and this prevented the disease extending in my own house, but the girl's mother who nursed her was attacked. She went home to be nursed, but had the disease in a much milder form, and one of her younger children, who took it from her, had it milder

This case probably not Typhus - 1. No evidence of exposure to Typhus - 2. No association with

still. It seemed as if the virulence of the fever was gradually subdued under the influence of a pure country air."

2. CASES OF CHOLERA AND DIARRHŒA.

Observation 37. In 1854, cholera was not known in the county of Bedford, when it broke out in the village of Ridgmount, and eleven cases occurred from first to last, all of which were fatal. On careful inquiry as to its origin, it was clearly ascertained that the first case occurred in a man whose son had died of cholera in London a week or two before, and whose clothes were sent down to the country. The poor man unwrapped the bundle of clothes himself, was seized with the disease and died. His case was the nucleus of the rest.

Observation 38. In Gravel lane, in the town of Bedford, where cholera was the most fatal in 1849, the stench from the drainage, cesspools, and privies, is sometimes almost insupportable. In one row in this lane eighteen persons died of cholera; the houses are back-to-back and without any ventilation. The pump is against the privy, and within six feet of the same pump are three privies and a dunghole. The well is eight or nine feet only from the pump.

Observation 39. In a house in the village of Cardington, several cases of diarrhœa of a dysenteric character, accompanied with rapid depression of the vital powers, occurred in all the children. One child died after six hours illness. My suspicions were aroused and I made a careful examination of the house, and detected an unpleasant smell in the back kitchen leading from the kitchen room in which the family lived. I found that the parents had often noticed a disagreeable smell, particularly on going down into the kitchen the first thing in the morning. On seeking for the cause of this I found that the wall in the direction of the sink was damp, and in the garden outside was a cesspool covered in, and full, close to the wall, the drain from which was choked. This receptacle contained the house-slops and matters washed down the sink for an unknown length of time. This state of things was remedied, and there has since been no illness in the family. The water-supply in this place was, and is good. The well is far removed from the cesspool, and remains unaltered in position.

Observation 40. Mr. Cox has given me the following history. "In July and August 1849, I was resident in the district of Poplar, and was intrusted with the principal charge of the poor. This district includes the Isle of Dogs. The western side of this oblong peninsula, rather

than island, called Millwall, and which is tolerably populous, nearly escaped the visitation of cholera. Not more than seven fatal cases in all occurred. In this respect it was more highly favoured than Poplar town, or Limchouse, where the mortality from the scourge was high for some weeks. There was no over-crowding on this portion of the island. On the eastern portion however, at that part which is contiguous to Blackwall, and which is chiefly uninhabited, were a few miserable and squalid looking cottages ; one-storied and low-roofed. They stood adjoining a brick-field in which their occupiers chiefly laboured. They were crammed from mud floor to roof with human beings, and on the average four or five adult persons lodged and slept in each room. The chambers were rarely more spacious than about ten feet square ; there was no thought of ventilation. In only one room in each house was there a chimney. The windows were small and not constructed to open. The fetid effluvium in some of the sleeping chambers was sickening, and precisely similar to what I have observed in densely-crowded lodging houses in Lancashire. In one room containing only 920 cubic feet, six full-grown men slept nightly. It is scarcely an exaggeration to say that this room was air-tight. There was scarcely a chink by which air could enter. The door was fast shut, as women occupied the next chamber. In a similar room in another of these

houses slept a man, his wife and child, and two male adults, a tattered curtain being for decency's sake drawn across the apartment, separating the beds. Instead of the proper supply of air, each person in this modern black-hole (not counting the child) was limited, I then calculated, to about twenty-four cubic feet an hour, of impure air; and this, supposing that at the commencement of the night the room contained none but pure air, and that the period of sleep was restricted to six hours, both very doubtful contingencies. The cholera one day entered, and assumed its utmost malignity in these wretched abodes. On the occasion of my second visit, I saw in one house three corpses, in another two, in a third two; all victims of the pestilence since the preceding day: at the same time five more were lying ill of cholera in adjacent cottages, three of whom subsequently died. In fine, of the population of these cottages one ninth was carried off in twelve days."

Observation 41. Mr. J. H. Houghton of Dudley, has forwarded me the annexed six illustrations. "At a place called Common Side there is a row of twelve houses detached from all other houses and buildings. In the front of them is a large garden and field; at the back of them a large field with a high road intervening; to the right and left the open country. A town with a large population is

within a quarter of a mile. In 1849, there were in a row, at about six yards distance from the front of these houses, three privies and twelve pigsties, rendering the houses intolerably offensive. In the epidemic of cholera in 1849, the inhabitants were attacked with the epidemic and nineteen persons died."

Observation 42. "In the month of October 1854, I was called to three cases of cholera at a place called Primrose Hill. The condition of the house was intolerable, within a few yards were eight pigsties in a most offensive condition. One of these was close under the windows where the miserable sufferers slept. The sewage from the pigsties formed large pools round about."

Observation 43. "In the same year and month a case of cholera, which ended fatally, occurred in one particular house. The house was detached and small; several persons slept in one room, and a pigsty was immediately under the bedroom window."

Observation 44. "In the same year, a sudden outbreak of cholera occurred at Sweet Turf, three persons being seized at mid-day, and two of them, a mother and child, being carried off within twenty-four hours. The husband kept his house offensive and dirty; there was no ventilation. The house was built on the side of a hill, and was very

damp. The drainage was bad, and a few yards off were two foul pigsties, the offensive matters from which were caught in a large pit, which was overflowing; the surface for twenty yards around it being covered with the poisonous fluid."

Observation 45. "A woman named Dimmock had been washing the linen of cholera patients. Her house was of the worst possible description, low, filthy, offensive, and in fact uninhabitable. She took cholera and died."

Observation 46. "At Netherton 'there was a block of houses of a triangular shape. The houses on two sides stood on high ground, and from the back doors of each row there was a great fall to the centre. As there was no drainage whatever, the refuse of nearly 150 houses, with pigsties, privies, and slaughter-houses, gravitated to the centre, along which an open ditch existed, three feet deep and two wide. This ditch was filled with a thick dark fluid, throwing off an intolerable stench. Along the sides of the ditch wells were dug, and the well-water charged with fluid from the ditch was used by the inhabitants for domestic purposes. In this place cholera appeared, and five deaths occurred."

Observation 47. For the following observations I am indebted to Mr. R. W. Watkins of Towcester.

“During the epidemic of 1854, the lower and badly drained portion of the town suffered severely with cholera. There were comparatively few cases in the upper part of the town, and a large proportion of those occurring in the upper part were clearly traced to local causes. In the highest and most airy part of the town there is a row of thirteen houses, principally inhabited by poor people ; in only two of these houses did cholera appear. In one of them the second case of cholera in the town occurred (the first being a quarter of a mile distant) ; shortly afterwards five cases of cholera and choleraic diarrhoea happened in the adjoining house. I was induced to make inquiries as to the existence of any local nuisance, and found immediately behind these two houses a privy, with a large cesspool so full that its contents were oozing up through the crevices of the floors and under the door-sills, the boards being nearly rotten.”

Observation 48. “In the lower and more unhealthy part of the town was a house in which we had almost always some one ill ; it was very low, one step leading down into it, and the height of the lower room was less than six feet ; an imperfect and ill-contrived drain passed by the door scarcely at a lower level than the floor of the house. As soon as cholera appeared in the town, I prognosticated publicly that this house would certainly not escape. Within

a month five of its eight inmates were attacked with cholera and four of them died. The father of the family shortly afterwards left the house by my advice, and it has never since found a tenant."

Observation 49. "In a 'yard' (or more properly a court) in the upper part of the town, were eight houses. In only one of these did true cholera appear, but in this there were three cases, one of which proved fatal. Within the outer walls of the premises, and close to the house door, was an overflowing privy, and the stench was almost intolerable in the common sitting-room. There was one case of choleraic diarrhoea in a house a few yards distant."

Observation 50. Dr. G. W. Spence of Lerwick, reports the next observation. "When I was stationed at St. John's Newfoundland, as assistant-surgeon of the Royal Newfoundland Companies in 1854, cholera prevailed epidemically, but all the cases occurring among the troops in my charge took place in one particular barraek, in one particular room of the barraek, and in one particular part of this room. On making an inquiry into the probable cause of this, I found that beneath the floor of this room, at that part where the infection first showed itself, there ran a drain which had not been carefully constructed; while opposite to this

part, at a distance of ten feet, was an entrance door to a long covered passage leading through the rampart to a privy which emptied itself into a cesspool. The men living in this room complained of a disagreeable odour frequently. In all other respects all the men in the barracks were situated exactly similarly as regards water and food, and yet not one was attacked with cholera who slept out of the room that was impregnated with the noxious air."

Observation 51. I will add one illustration of the local generation of diarrhoea, supplied to me by Mr. W. R. Milner. "When making my monthly inspection," says Mr. Milner, "on the 1st of March 1848, I found that the basements of two of the four wings of the New Prison (Wakefield) were flooded, but the basements of the other two wings were not flooded. The two wings which were flooded, were B and C, and contained three hundred and ninety prisoners; the wings which were not flooded were A and D, and these contained three hundred and thirty prisoners. It was found that the flooding was caused by the overflow of a drain, arising from the drain having become stopped at its outlet by a plank which had been left in it by the workmen when the drain was constructed. The drain was freed, and the water got rid of. So long as the ground was covered with water no disease

appeared, but as soon as it began to dry, cases of diarrhœa occurred, and during the month forty-three men were placed on the sick list for that disease in B and C wings; while, during the same month, only two cases of diarrhœa occurred among the prisoners in A and D wings. The forty-three cases in B and C wings were placed on the sick list on the following dates."

| Dates. | Cases. | Dates. | Cases. | Dates. | Cases. |
|-----------|--------|------------|--------|------------|--------|
| 4th | 1 | 14th | 3 | 24th | 2 |
| 5 | 1 | 15 | 2 | 25 | 1 |
| 8 | 5 | 16 | 4 | 26 | 1 |
| 9 | 6 | 17 | 1 | 27 | 2 |
| 10 | 9 | 20 | 2 | 29 | 1 |
| 13 | 1 | 23 | 1 | | — |
| | | | | | 43 |

3. CASES OF REMITTENT FEVER.

Observation 52. I am indebted to Mr. Watkins of Towcester for the subjoined facts. "In the year 1846, a farmer's wife aged 60, came under my care with remittent fever. The attack commenced about the beginning of January, and terminated in the middle of March. In 1847, a precisely similar attack came on in the early part of October and continued till the end of the following January. In 1848, the patient was again attacked in October, and this time the disease continued with varying intensity until March. The locality was a village near the top of a hill, with

free currents of air in every direction. The soil a stiff clay. No similar case occurred in the village during either of these attacks. The only cause which could be assigned for the recurrence of this attack, in the winter, was that the house-drain after passing underground for a few yards opened into the farm yard in front of the house. During the winter months this yard was occupied by cattle, and the flow of sewage was necessarily obstructed by the accumulation of litter and manure, and by the treading of the cattle. This opinion was pressed upon the husband by myself, and confirmed by an eminent physician who was in consultation with me during the case. The farmer, however, an ignorant man of the old school, obstinately refused to admit this explanation. 'It always had been so since he was a boy, and no harm had ever come of it.' During the third attack the poor old man died suddenly, and at the end of the spring the farming stock was all sold. The yard was no longer used as a farm-yard. The drain still remained uncovered, but the sewage had free course down the yard, and into the ditch by the public road. It is remarkable that *for seven years afterwards this patient did not have a recurrence of fever*, but about the end of that period, circumstances (which I need not particularize) required that the yard should again be used as a farm yard, and it is no less remarkable that in January 1856,

she was again attacked in a similar manner; the symptoms however were not so violent, and were alleviated in a month. In the following October the disease returned, presenting the same character and the same intractability that it had done in former years, and in this instance it continued the whole of the winter. A change of residence was earnestly recommended as the most effectual means of removing the disease, but unfortunately it could not be effected."

In April 1859, I received a note from Mr. Watkins, in which he informs me that after the last attack, an underground drain had been laid from the house in which the patient lived, through the farm-yard into the public road, and that she had enjoyed very good health during the subsequent winter. Mr. Watkins justly remarks, "my previous suspicions of the origin of the fever have certainly been confirmed by the result."

Observation 53. The next example of the origin of remittent fever is supplied by Dr. Spencer Thomson. "A lady who had been on a visit to a country town, returned home suffering from all the symptoms of remittent fever, the attack lasting severely for eight days. The only assignable cause for this attack, and this cause was sufficiently well marked, was that while she was on her visit, a stagnant pool situated close to the house in which

she resided, was in process of being emptied and cleansed, the mud and decomposing matter being thrown out on the banks of the pool. The pool had long been the receptacle for a part, at least, of the town sewage. The decomposing refuse thus thrown out of the pool was left exposed to the heat of the sun in a hot season; and, to the inhalation of the emanations arising from this source my patient unhesitatingly traced her malady, which quickly passed away on removal into another air."

4. CASES OF SCARLET-FEVER.

Observation 54. Dr. Richardson has favoured me with the details of a remarkable instance in which scarlet-fever was propagated by communication. "The disease raged in a village, and carried off many victims among the younger population. A man residing at the extremity of the village, and on an eminence, had a child seized with the disorder. He had three other children, the whole of whom were at once sent away to their grandmother, who was residing at another village eight miles off. The child attacked with the disease suffered from it in its malignant form and died. This was the last case that occurred at that time in the village. At the suggestion of Dr. Richardson, none of the other children were recalled for eight weeks, and the house was, meantime, thoroughly cleansed and

whitewashed, and the bed linen with every other material that could be considered capable of retaining the disease was purified. At the end of two months one boy returned home; he had not been in the house a day, before he was seized with the premonitory symptoms of the disorder. He had the disease in its severest form, and in spite of all treatment, died. A period of four months elapsed, in which interval the same cleansing processes were carried on as before; and the parents, anxious to have their children back again, and thinking that now all danger must necessarily be past, recalled their last remaining boy to them. There had been no case in the village during the term intervening, but the boy on returning home was seized exactly in the same manner as the previous one, and also died. What became of the last child of these unfortunate people the writer does not know, as he left the neighbourhood soon afterwards. Dr. Richardson is of opinion that the thatch in the roof of the cottage was the retaining medium of the poison."

Observation 55. Dr. Richardson has furnished me with the history of another case, as follows: "He had attended a lady in confinement who had just returned from the Continent in perfect health. The confinement was normal and the infant born healthy. The lady's recovery was rapid, but on the third day a fresh nurse was called to her. Within

twenty-four hours after this woman's appearance in the house, the lady, her infant, and an elder child, a boy, were seized with rigors and other premonitory symptoms, which lapsed into well marked malignant scarlet-fever. The lady recovered, but the infant died. The elder boy recovered after many months of lingering suffering: an enlarged gland in the neck suppurated; the suppuration extended to the cheek, and the cheek itself was left paralysed. These three patients all became ill at the same time, and evidently from the same cause; for on making enquiry, Dr. Richardson discovered that immediately preceding her attendance on the patients, the nurse had been nursing, temporarily, three children in a neighbouring town who were lying ill with scarlet-fever. To how many other patients the same nurse conveyed the disease it is impossible to say. For some weeks afterwards the disease accompanied her, indeed, wherever she went, and a continued and fatal epidemic which infested a large village seemed clearly traceable to her as the first bearer of the poison."

Observation 56. Mr. Cox has written me the following illustrations: "In a village wherein I am at present resident there was an epidemic of scarlatina last year. The fever assumed a mild character, except in two houses, both farm-houses; in one of these, three children died out of four attacked, in

the other, one of three. The malady was brought to the village from a neighbouring village-school by one of the children. I visited these two houses. I found them decidedly insalubrious, from defective drainage, and from the constant presence of dung-heaps, fetid rubbish, and black pools of stagnant water in the yards and courts, both in the front and in the rear of the premises. There was perpetually an offensive smell from these causes. I examined the water from a well on the premises of the largest house. Notwithstanding the depth of the well, the water was impure and charged with the nitrates, evidently from the surface water of the courts and yards finding its way through the soil. None of the poorer class of houses in the village are so ill circumstanced; and none suffered like these during the epidemic. The evil agencies in this case appeared to have changed the type of the malady from a mild to a malignant form."

Observation 57. The following, a striking example of the transmission of scarlet-fever through clothes, is communicated to me by Mr. Augustin Pritchard of Bristol. It occurred in his own family. "My eldest boy," he states, "eight years old, went on a visit to a distant part of the country and caught scarlet-fever, a man-servant in the house which he visited having had it before, and there being a few scattered cases in the neighbouring village. I fetched

the boy home after the eruption disappeared, but would not allow him to communicate with his brothers and sisters for full two months. About that time a servant girl in my house was set to work to dust and brush a great-coat in which my son had been enveloped during his journey home. A day or two afterwards she had a severe sore throat, which we attributed to cold. On the 2nd of May one of the children slept in the same bed while she was ill, but was separated from her after that night. On the 12th the child had fever, followed by the eruption of scarlet-fever. Two other of my children were soon seized in a similar manner, but all got well. This was evidently a clear case of communication through the clothes, as there was not another case in the neighbourhood, and had not been for a very long time."

Observation 58. I have the authentic record of an unpublished case, where scarlet-fever was conveyed by a letter, and led to disastrous results. A gentleman well known in the scientific world, had some friends living at some distance, whose family were suffering from scarlet-fever. The gentleman had given strict orders that all his letters should be brought to himself personally. One morning, however, his child transgressed the rule and carried a letter to her mamma, who read it and sent it down by the child to her husband. The

letter was from a member of the infected family, and there was no other kind of communication between them, and no disease in the neighbourhood. The mother and child took scarlet-fever simultaneously, and both fell victims to its virulence.

Observation 59. In the summer of 1848, one of my own family, a girl aged six, went to pay a visit in the county of Hertford; she complained of sore throat the first night after arrival, which was quickly followed by the rash of scarlet-fever. She passed through rather a severe attack of this disease, and on recovery from it had erythema nodosum upon the legs; anasarca and albuminuria then made their appearance, and her convalescence was slow. I ascertained that a child in the same house, a fortnight before, had suffered from slight sore throat, with a faint rash which had been thought to have been measles, and was so slight in its symptoms that the child had not kept its bed. We kept up for several weeks a very strict quarantine, and the disease spread no further.

5. CASES OF ERYSIPELAS.

Observation 60. Dr. Richardson has favoured me with the following singular history. "In the last month of the year 1847, three men residing in an agricultural neighbourhood, in a village near to

Saffron Walden in Essex, were engaged thrashing corn in a barn. They went to work in the most perfect health, and no epidemic of any kind was prevalent at the time. Erysipelas had not been known in the village for many years. The men had approached the end of their task, and were turning over the last bundles of corn, when, at one moment almost, they all three felt a peculiar sensation, which they called "a sickening sensation." They thought nothing of it at the moment, but in the course of an hour they agreed that they were all too unwell to continue at their work. They felt cold, and shivering, and sick. They sent for their employer, a retired medical practitioner, who, struck by their symptoms, recommended them to go home and to bed. The author of the case saw these men in the evening of the day, and found them labouring with all the symptoms of a threatened febrile attack. He prescribed for them a saline. On visiting them on the following day, each one was found suffering from erysipelas of the head and face. They were located in different houses, and had no communication with each other after their first separation. In one the attack was very slight, and the recovery took place in two or three days, the erysipelas confining itself to one cheek and ear. The second man was attacked very severely, but recovered. The third, the eldest of the three, a man upwards of 60 years of age, was laid up for many weeks, and

his life was at one time despaired of. The erysipelas extended over the whole of the right side of the scalp, pus formed beneath the scalp, and free incisions had to be made. Recovery took place ultimately. These men were located in small cottages, thickly inhabited; but it is remarkable that there was no extension whatever of the disease from them."

Observation 61. The succeeding cases point out the possibility of propagation of erysipelas by personal intercourse. They were also communicated to me by Dr. Richardson. "A man 53 years of age, who had led a steady life, and had been engaged in agricultural pursuits from his childhood, suffered from necrosis of the second finger of the right hand. The disease of the bone was very extensive, and it was considered advisable to remove the finger at the carpo-metacarpal articulation. The man resided in a small village, which was at the time free from any kind of epidemic, but there had been in the place an epidemic of continued fever some short time before. The finger was removed at the man's own cottage by the friend of the author, in whose practice the case occurred. Dr. Richardson administered sulphuric ether to this man, chloroform not having then been used as an anæsthetic. The operation was skilfully and rapidly done, and for a week everything went on favourably and the wound

was nearly healed. At the end of the week a son of the patient came over to see his father from a village, eight or ten miles away, in which erysipelas was epidemic. The son himself had suffered from erysipelas of the head and face, and still bore some desquamatory evidence of the disease. On the morning after the arrival of this young man, the father was suddenly seized with rigors, which were soon followed by pain, heat, and redness in and around the nearly healed wound, and assumed a spreading erysipelatous character. The disease crept along the whole of the limb, involving the shoulder, and the case ended fatally five days after the commencement of the erysipelatous symptoms."

6. CASE OF SMALL-POX.

Observation 62. I am further obliged to the friend immediately named above for the following instance, in which small-pox was communicated to a neighbourhood by the emanations from a small-pox patient. "In the autumn of 1847, in a country town, a young man, the son of a miller, returned from a town many miles off, very ill and unable to work. His father lived at a mill which is situated on an eminence about a sixth of a mile from the town. A hundred yards or so from the mill and running down the descent, was a double row of cottages, neatly built, and inhabited by

clean and industrious people. The illness of the youth terminated in confluent small-pox, and ended fatally. From the miller's house there was a surface drain which ran from the mill down by the cottages, nearest to those on the left hand side, the rows of cottages being separated by a road at least ten yards wide. The miller's son died rapidly, and his clothes were destroyed, but the excreted matters from his body with other refuse from the house were poured down the drain, in their way to the cesspool which was at the bottom of the hill. In the course of two or three days, there was scarcely a child in the cottages on the left hand side that was not prostrated with the disease, and as but few had been vaccinated, several died. One or two cases occurred on the right hand side, but these were very limited in number as compared with the others. The houses being isolated from the town, the parish authorities were enabled to set up a strict quarantine. At the suggestion of their medical officers, two men from the workhouse were placed on the road between the town and these cottages, and allowed no child, woman or nurse to enter the town, a messenger being appointed to make purchases for them. The result was that the disease was confined almost entirely to this particular spot. One or two cases which did occur, in every case by intercommunication, were also placed under supervision, and the town was

entirely saved from the further ravages of an epidemic which had once or twice proved most fatal in every part.”

7. CASES OF PUERPERAL FEVER.

The disease puerperal fever was not specifically indicated in the terms of my original essay, but as I look upon this disorder as belonging to the contagious class of diseases, I make no apology for inserting the following case.

Observation 63. In the year 1850, a young unmarried woman at the village of Cople suffered from peritonitis with perforation of the intestine, a form of disease which I have met with four times in my locality. The case was rapidly fatal, and a young medical gentleman who was at the time residing with me made a post-mortem examination. The operation was done neatly and cleanly. I was present myself on that occasion, but took no part in the operation. The smell was very offensive, and I noticed afterwards that the same smell was communicated to my excreta from the bowels, a concurrence which has been remarked upon by Dr. Brinton as following post-mortem examinations. The young man returned home, and the next day attended for me a woman in labour. He wore the same clothes as on the preceding day. The labour

was natural, and took place at the distance of seven miles from the previous case. On the third day after labour the woman was seized with puerperal peritonitis, and died.

Observation 64. Two days after attending the previous labour, and before the puerperal symptoms had presented themselves in that case, the same gentleman unfortunately attended a second case at a place five miles off in another direction. He wore the same clothes as before. On the third or fourth day this poor woman was also seized with puerperal peritonitis and died. Her labour had been quite natural.

Observation 65. The mother of the last named patient, a feeble woman of a spare habit, nursed her daughter after her confinement, and washed the linen after her decease. I failed to ascertain if she had previously wounded her hand or fingers, or if she received any prick or scratch in those parts in the act of washing; but within two days after having washed the linen, the absorbents of the right arm became inflamed, followed by enlargement and inflammation of the axillary glands. Time was not allowed for suppuration, for the vital powers became rapidly depressed, and she sank within two days, in spite of all remedial measures and the free administration of stimulants.

Observation 66. The occurrence of these disastrous cases induced me to suspend for a time the obstetric duties of the medical gentleman referred to. This decision was the result of a conviction that he had been the innocent medium of communicating the disease, directly in the two first cases, and indirectly in the last case related. This gentleman had been in the habit of keeping his finger nails very short, and within three days after the occurrence of the last case the fore-finger of the right hand became inflamed under the nail and very painful. The inflammation extended along the hand and absorbents of the arm to the axillary glands, which were much enlarged. The constitutional disturbance was great, and he was confined to his bed for a week, and unable to go out for more than a fortnight from the commencement of the attack. Ultimately he recovered.

CHAPTER XI.

AN ANALYSIS OF THE FACTS PRESENTED IN THE
LAST CHAPTER.

Origin and Spread of Diseases. Absorption of Poisons. Concentration of Poisons. Effects of Heat and Cold. Ozone. Conditions of Body, Hereditary and Individual. Fear.

The observations recorded in the last chapter, though less numerous than they might have been, are not the less instructive. Indeed, for the investigation of the subject now before us, numbers of examples are not required, but exactitude in the narration of such examples as are given: for, as in the laboratory two or three determinate experiments are sufficient to enable a chemist to demonstrate a series of results with precision, so in the observation of disease two or three occurrences absolutely observed may lead to the demonstration of a whole series of causes and of their resultant phenomena.

The first inference deducible from the observations is, that in every one of the diseases noticed in our list, the disorder was traceable to a specific poison. To argue for the existence of these spreading

distempers as dependent on meteorological conditions, or even on mere offensive smells, per se, is, with the facts we have seen, mere waste of time, the mistake of making a coincidence take the place of a cause. As certainly as in the vegetable world we trace one particular plant from its own seed, so in these epidemics we are led to trace, as clearly, the origin of each from its own particular exciting poison. But between the growth of plants and the origin and growth of diseases of the epidemic type, there are essential differences as well as essential analogies: and I dwell on this point because, by mere figure of speech, it has become a common rule to draw conclusive comparisons between vegetative growth and disorders of the kind now under consideration. The difference is this,—in vegetation every plant produces its own specific germ; in disease, some particular affections produce the same affections, evidently and obviously by the dissemination of the same poison, as for example, scarlet-fever and small-pox; but in other cases it would seem that the poisons may arise in decompositions taking place outside the body, which poisons, carried to the body, excite disorders specific in kind, and afterwards perhaps communicable. Illustrations of this nature are given in cases of typhus and typhoid fevers. In these, as it seems to me, the poison is not necessarily first produced in the body of an affected person, and transmitted as necessarily therefrom in order that

it may spread to another body, but is rather to be considered as originating in certain states of animal and vegetable decompositions, as being primarily given off from them, as passing from its place of origin into an organism, as increasing there, and as becoming from it transmissible to other organisms by communication. Further, the observations tell to us a fact almost new, viz., that in the course of certain diseases not considered communicable, secretions are formed which if placed under favouring conditions become the essential factors of disease. How clearly this is demonstrable may be proved by reference to observation 63, page 162, where, in a patient dying from simple peritonitis, a poisonous secretion was afforded, which infecting the hands of the post-mortem operator, was communicated by him to parturient women, and excited a specific contagious disorder known as puerperal fever.

Once more, we saw proved in observation 60, page 157, that another disease, erysipelas, may have its origin in vegetable poison: as though the exciting poison of the disorder lay harmlessly stored up in the vegetable dried products, ready on being set at liberty, and on coming into contact with a living structure, to exert all its evils and expend them on the animal organization.

Thus we see at least four distinct modes by which the poisons of the communicable diseases originate

and spread. To the first class belong those diseases which develop in decomposing organic matter, in the cesspool, the manure heap, and the thickly inhabited court or alley. To the second class belong those which spread by the transmission of one particular poison from one person to another. To the third class we trace those which spring from the absorption of matters secreted in the course of common diseases. In the fourth class we observe the disorder springing, as it were, directly from what seems to be dead vegetable matter.

I doubt not but that in course of time these views will undergo a modification, and take an approach to an unity, real or ideal, which does not now exist: but I have thought it safest at this present moment to use the facts as they are presented to us, without too severe an attempt at reducing them to one efficient source and one typical form.

Passing from the consideration of the poisons—as poisons, we learn from the observations recorded, that the absorption of the poisonous matters by the different inlets of the body is not confined to one surface, but may in fact occur at any surface to which the poison shall be applied. In the cases of typhus fever the reception of the poison may be by two surfaces, by the respiratory, or by the alimentary, and indeed it is very difficult to determine whether the absorption is confined to one of these. If my observation be correct, I infer that in some instances the

infecting matter is borne into the circulation by the lungs, while in other cases it is carried in water, and let into the blood by the mucous membrane of the intestines. The difficulties in the way of explaining the mode of absorption are truly very great; for while we can fully understand the introduction of an organic substance by the process of inoculation, that is to say by direct introduction into the blood, it is much more difficult to comprehend such absorption when it has reference to the imbibition of the same organic body through a membrane. However, this is clear so far, that there may be absorption through a membrane, and so much granted, it is easy to see that absorption is as readily performed through one membrane as through another, as readily through the mucous surface of the lungs as that of the stomach, or through the lining of the uterus, or the conjunctiva of the eye, as through any other part where nothing but epithelium protects the outer and exposed structure.

There is another point to which our attention must be directed, and which has reference to the conditions that favour the spread and the action of the different poisons. Our preceding observations on these matters are in some respects very valuable, in others imperfect. They teach us in the first place clearly, that in every case the concentration of the poisons in the ill ventilated room, the cesspool or the drain, is at all times attended with mischiefs

and dangers which, notwithstanding the actual existences of these poisons, might be prevented if they, the poisons, were not allowed to accumulate and concentrate.

In the next place, we may I think safely infer that some conditions of the atmosphere materially favour the spread of the various poisonous bodies to which our attention has been directed. Extremes of cold and extremes of heat, both according to my view prevent the action of these agents; the heat probably by destroying or rapidly removing them; the cold also by destroying them or by rendering them inactive for the time. On the other hand, a moderate temperature, such as a mean of 60° Fahr., seems to exert, distinctly, a favouring influence, and this especially when the air is surcharged with moisture. The evidence indeed seems to lead to the idea that some of the organic poisons, if not soluble in the water of the air, are at least suspended by it, and are as it were borne by it to the pulmonary absorbing surface.

With the exception of the above named meteorological states, the condition of the atmosphere does not seem materially to influence the spread of the communicable diseases referred to in the preceding chapters. It is a matter for future observation certainly, whether the presence of ozone may exert an influence in neutralizing the effects of certain of the poisonous gases. Of one thing it is

quite certain, that in the neighbourhood of cesspools all evidence of the presence of ordinary atmospheric ozone is lost, as though it, the ozone, were being expended in overcoming or resolving the emanations arising from the decomposition of the cesspool. Further, it is certain that the absence of the ozone in these localities does not depend on the absence of it in the air surrounding, as is proved by the circumstance that on simply making an ascent the ozone appears. Thus if we take the ozone test paper into an ill ventilated close building, say the body of a badly constructed church when it is full of persons, the paper will give no reaction, any ozone present being either employed in deodorising, or being suspended in its chemical action. But if from the body of the church we simply mount to the battlements and there expose the test paper, we may get marked evidence of the presence of the active oxidizing agent. Now we know as a collateral fact that many of the communicable diseases, such as typhus and typhoid fevers, progress towards a fatal termination with greater rapidity in crowded dwellings than they do in open and elevated places, and we might therefore infer that the absence of the active oxygen in one case, or its presence in another, afford a reason for the differing results. It may be so, and in respect to certain of the diseases under discussion, especially the two diseases named above, I believe it is so ; nay I have evidence from my own

experience that the diffusion of ozonized air through the apartments of persons suffering from these disorders is of immense service, in that it keeps the room free of oppression and effectually destroys the offensive odours arising from the gaseous excreta of the subject. But in saying this it must not be too hastily concluded that the same line of treatment will answer equally well in every case of communicable disease. For the truth lies in this, that the communicable diseases divide themselves into two groups in a pathological point of view, one of which groups is marked by the acute inflammatory type, the other by a low adynamic type: the first indicating a rapid oxygenation of the tissues, the second a reduced oxidation. While therefore it might be, and indeed is, useful to employ active oxygen in cases of the adynamic series, it might be equally as deleterious to employ it in cases of the acute series, a suspicion borne out very considerably by the experimental fact, that the exposure of an animal to an atmosphere surcharged with ozone, or I had more correctly said super-ozonized, leads to acute inflammatory mischief of the mucous membrane of the pulmonary tract. I have sketched out these points rather as suggestions for thought than as assumed facts, or even assumed theories. They have often occurred to my mind very forcibly; and so I write them down here, where if anywhere they are strictly in place.

There is one other subject to which I must refer before closing the chapter. I must briefly discuss the often debated point, whether there are conditions of body which render the system more susceptible of diseases that are communicable, at one time than at another. I confess that I am not competent to make up my mind on this point in all its entirety. That there are hereditary tendencies or proclivities to certain special diseases ; that there are apparently hereditary constitutional conditions which tend to ward off particular diseases, seems to be attested by common experience : that there are certain low or depressed conditions of body which favour the influence of poisons communicable in kind, and certain other conditions of system which oppose the effects of the same poisons appears also to be proved on the ground of common experience ; I think I have myself seen evidences of this nature, and have corroborated their truth by a rigid analysis of all the obtainable and reliable data. But the cases so presented are obviously very few in number, and pertain to a limited list of diseases. I believe, speaking from experience exclusively, that small-pox, erysipelas, and scarlet-fever, are modified by hereditary states : that in some persons, that is to say, there is hereditary proclivity to these diseases, while in others there is an hereditary endurance against them. There are again diseases which are not, as far as I ever witnessed, at all modified by

hereditary conditions ; these diseases being whooping-cough, measles, croup, typhus, typhoid, and cholera. The influences of individual bodily states in reference to the diseases named, pertain I think mainly to the typhus and typhoid fevers. I can scarcely accept that either small-pox, scarlet-fever, cholera, whooping-cough, measles, croup, or diphtheria, are particularly influenced in reference to their propagation by states of the body. It is clear that these diseases attacking a feeble body, will lay that body prostrate quicker than they would another body having greater powers of resistance ; but the susceptibility to receive the poison would I assume be much the same in each case. In a word, as a strong and a weak man are each equally liable to the effects of a poisonous dose of hydrocyanic acid, so are they liable to the poisons of such diseases as cholera and scarlet-fever.

Further, I have but little confidence in the theory that mental fear has much to do with the communication of contagious disorders ; frightened people take the maladies in their fair share, and no more, with courageous people, and perhaps being attacked they succumb more quickly. But I no more accredit that a man can frighten himself into scarlet-fever, small-pox, or cholera, than that he can frighten himself into the symptoms of snake-bite poison, or primary syphilis.

Yet amongst the people it is one of the most common statements, in regard to an epidemic

malady, "he frightened himself into it:" and in regard to cholera, I have heard this statement until I am weary of it. Suffice it to say, that it is destitute of all foundation, and that the analysis of every case of such assumed kind resolves the affection either into no specific disease whatever, or into a specific disease traceable back to a specific cause.

CHAPTER XII.

ORIGINAL EXPERIMENTAL INQUIRIES ON THE TOXIC
EFFECTS OF GASES EMANATING FROM SEWERS
AND CESSPOOLS.

Experiments with Cesspool Air. Sulphuretted Hydrogen. Sulphide of Ammonium. Carbonic Acid Gas.

While accepting, for the reasons previously stated, the belief that a special poison is requisite for the production of every spreading disorder ; while recognizing the poisons themselves as of organic character and development ; and while disconnecting them altogether from common inorganic poisons, I thought it might not be an unprofitable task to perform a few experiments with sewer-air, and with some of the gases usually found in the neighbourhood of cesspools.

The gaseous emanations from cesspools have not escaped the attention of chemists. Analyses of sewer-air have yielded evidence of sulphuretted hydrogen, sulphide of ammonium, carbonic acid, nitrogen, sometimes of phosphuretted hydrogen, and various organic living products. Dr. Odling has stated that sewer-gas has an alkaline reaction.

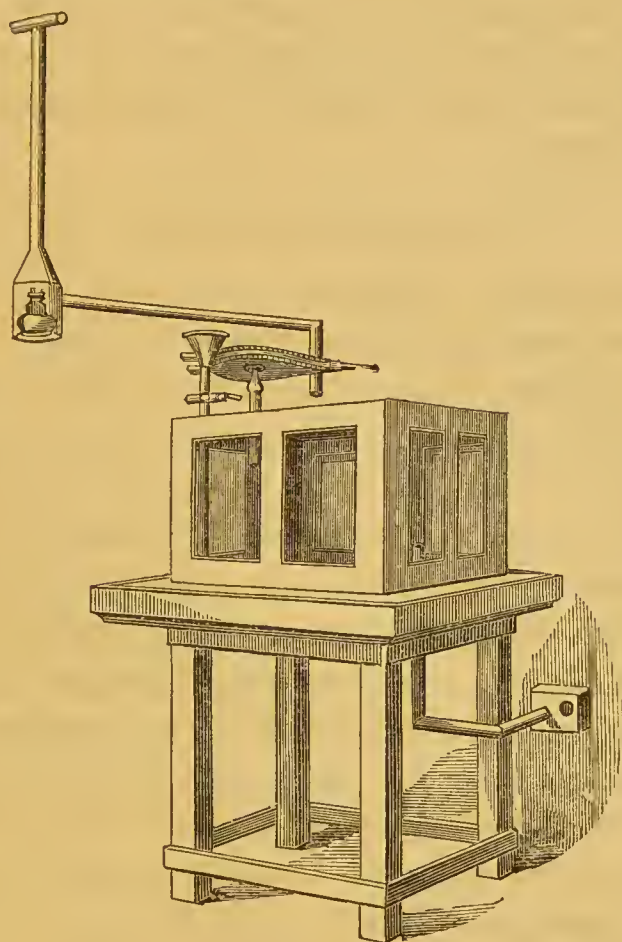
For the purpose of experiment I selected a large cesspool, communicating directly with a privy, and receiving together with the animal excreta, the liquid refuse of an inhabited house. The cesspool was full, and had at all times a bad odour, but during hot weather the smell from it was almost intolerable. The inhabitants of the house had not suffered from any epidemic disease for several years ; nor did the presence of the sewer affect their general health. The well supplying the house with water was removed several feet from the sewer, and was so protected that the supply was uncontaminated. Close to the sewer I constructed a small room, which I fitted up as a temporary laboratory. Two gutta serena pipes rather more than an inch wide were carried into the cesspool, and terminated in two large inverted funnels a few inches above the level of the contained soil. The other terminal ends of the tubes opened in the laboratory and were so arranged that they could be opened or closed as required.

By a bellows affixed to the free end of one of the tubes in the laboratory I was able at any time to draw off the cesspool air, and subject it, in current, to chemical inquiry. I examined it on numerous occasions when the weather was hot, and the emanations were offensive, as well as when the weather was cold, and the emanations were scarcely perceptible. Occasionally the reaction of the cesspool gas was feebly alkaline, but more generally

neutral. Evidence of sulphuretted hydrogen, and sulphide of ammonium, and of carbonic acid could at all times be elicited in smaller or larger quantities; and these, with the common air with which they were diluted, were as far as I could find, the only constituents except ammonia, which now and then was present so as to give the alkaline reaction. After carrying on these inquiries for some weeks, I constructed a box of glass and wood, as shown in the drawing. The box had a clear space within of 5832 cubic inches; and such arrangements were made that sewer-air could be constantly drawn through it, and an animal placed in it could be supplied with food without the ingress of atmospheric air. The form of the chamber, the mode of introducing food, and the other arrangements were copied from a similar chamber invented by my friend Dr. Richardson, for his experiments on the properties of oxygen.

In order to keep up a current of sewer-air through the box I introduced one of the gutta percha tubes into it, at the lower part; from the upper part I carried a tube in the form of a small chimney,—as represented in the drawing. At the point where the tube made the right angle upwards it was expanded into a conical shaped box, in which a spirit lamp was placed, so as to create an upward draught. The arrangement worked exceedingly well, and a current of air from the cesspool, through the

box, and upwards through the ascending tube was kept steadily going.



I also arranged so that a pair of bellows could be attached to the box instead of the flue-tube ; a pipe fixed to the valve hole of the bellows was inserted into another tube coming from the box, as shown in the drawing. By this arrangement when the bellows were in play, the cesspool air was kept constantly passing through the box, and out through

the nozzle of the bellows. By means of india-rubber tubing attached to the nozzle I was also enabled while the experiment was going on to collect some of the sewer air, and ascertain to what gases the animal was being subjected at the moment of observation.

1. EXPERIMENTS WITH CESSPOOL AIR.

Experiment 1. Effects of Cesspool Air. A young dog was placed in the chamber at 12 o'clock noon: and a current of the cesspool air passed constantly through the chamber by means of the chimney draught. Half an hour afterwards the animal became very uneasy and restless, vomited, and had a distinct rigor. In the course of the day he was freely purged, with tenesmus and slight protusion of the rectum. At 12 at night I left him with the opening at the upper part of the box disconnected from the flue-tube, so as to allow the admission of air. In the morning he was removed. He was somewhat exhausted. The purging and vomiting had ceased, but he refused food for some hours. He recovered.

Before the animal was placed in the box, I tested the air from the cesspool. Its reaction was neutral, it yielded evidence of sulphuretted hydrogen.

Experiment 2. Effects of Cesspool Air. Another dog was placed in the box connected with the

cesspool, and a free current of air from the cesspool was kept up with the bellows. In ten minutes the creature became very uneasy, and soon afterwards vomited and was freely purged, with tenesmus. I did not remove him from the box for a period of five hours. After the vomiting and purging, he suffered but very little, and after removal quickly recovered.

Experiment 3. Effects of Cesspool Air. A mouse placed in a cage was let down into the cesspool to within three inches of the surface of the contained soil. The cesspool was freely open above, so that there was no exclusion of air. The animal was also well plied with food.

After this exposure for a day the animal seemed lively and well, and took his food heartily. On the three following days, the same. On the next day I found him dead and rigid.

Experiment 4. Effects of Cesspool Air. I placed another dog in the box, already described, connected with the sewer by the gutta percha tubes. The cesspool air was kept constantly in current through the box by means of the bellows, which were gently but effectually applied for a quarter of an hour every hour during the day. A free current of the sewer-air was kept up during the night by means of the chimney. Food was also

liberally introduced. Thus conducted, the experiment was continued for twelve days, with such brief intermission only as sufficed for rapid cleansing of the box. During the first day the animal was restless and uneasy, and refused food. On the second day, soon after the bellows were put into play sickness came on, which was repeated frequently during the day. In the afternoon the animal was much purged, the evacuations being very watery. After this he was thirsty and restless. On the third day, in the morning, he shivered for some time, and he refused all food; the feet were somewhat swollen, and the under surface of the paws very red: towards the evening he slept, but with a peculiar kind of shivering with each inspiration. On the fourth morning he was found to have eaten some paunch during the night, and he drank some milk: he slept during the forenoon, but was restless towards evening. On the fifth and sixth days he was much the same. On the seventh day he was restless and purged; he ate no food. On the eighth day he ate no food and was restless. He was now much reduced in flesh. On the ninth day he ate no food, and seemed very ill and miserable. I took him from the box while it was cleansed, and offered him food which he ate voraciously and to repletion. When removed from the box the skin seemed to be preternaturally hot and dry, and he was very weak; his gait somewhat feeble. After taking

the food he was removed again into the chamber and plied with the cesspool air. On the tenth day his appetite was better, but he was sick and purged in the evening. On the eleventh day he was very restless, barking and howling piteously ; his appetite falling off. On the twelfth day he was much the same and very ill, I had him therefore removed to his kennel. He walked feebly, but soon after his liberation ate heartily of food. This dog continued very thin, and was feeble for six weeks after his removal from the box.

2. EXPERIMENTS WITH SULPHURETTED HYDROGEN.

Experiment 5. Effects of Sulphuretted Hydrogen.

A puppy was placed in the chamber, and 100 cubic inches* of sulphuretted hydrogen were introduced. The breathing instantly became laboured. In two minutes the animal fell insensible on his side, and in another half minute he died without a struggle. On making a post-mortem examination an hour after death, I found the right side of the heart filled with fluid blood to distension. In the left side the blood was partly eoagulated. The fluid blood coagulated quickly when received into a glass. The corpuscles of the blood were natrnal. The lungs were congested in the lower lobes and posteriorly. Above

* Or 1.714 per cent., as diffused through the air in the box.

they were pale and free from congestion. The stomach and abdominal viscera were healthy. The vessels on the surface of the brain were slightly congested.

Experiment 6. Effects of Sulphuretted Hydrogen.

Another puppy was placed in the box as before, and twenty-five cubic inches* of sulphuretted hydrogen were driven in. In three minutes the animal fell on his side insensible. In this condition he lay for an hour without any indication of pain, but with catching respiration. At the end of an hour he ceased to breathe. The post-mortem examination was made directly after death. The lungs were generally pale and free from congestion. The right side of the heart was filled to distension with blood, the left side contained fluid blood. The blood coagulated in eight minutes after being removed from the body. It was dark in both cavities and the corpuscles were irregular; they floated about freely between the slips of glass but not one was natural. Some were crenated at the edges, others shrunken and broken up. The stomach presented nothing unnatural. The vessels of the brain were congested.

Experiment 7. Effects of Sulphuretted Hydrogen.

A hedge-hog was put into the box, and twelve cubic inches† of sulphuretted hydrogen were introduced.

* Or 0.429 per cent.

† Or 0.206 per cent.

He remained curled up, and the only difference perceptible was in increased frequency and depth of the respiration. In two hours the breathing was rather laboured, 35 per minute. In four hours the respiration was 60 per minute. This afterwards improved, and he was removed in five hours and a half apparently well.

Experiment 8. Effects of Sulphuretted Hydrogen.
At 4.27 p.m. a dog was placed in the chamber, and twelve cubic inches* of sulphuretted hydrogen gas were slowly introduced. Within a minute he fell on his side and was seized with tremors. The action of the heart became irregular, and within four minutes the respiration had apparently ceased. This cessation of respiration continued for about two minutes, when he began to breathe heavily. The respiration next became very quick and catching. Afterwards the quick respiration came on in paroxysms, with an occasional long-drawn inspiration. In three quarters of an hour from the commencement of the experiment the respirations were 112 per minute, rising sometimes to 120 per minute; they then became deeper and stertorous. I removed the dog from the box at fifteen minutes past six, having exposed him to the diluted gas one hour and forty-eight minutes. The respirations were at this time stertorous, the limbs were rigid, and the

* Or 0.206 per cent.

head was drawn backwards. The respirations gradually became more feeble and catching, as if solely diaphragmatic, attended with a kind of hiccup. The body was universally cold. There was subsultus tendinum. The respiration then became very peculiar, consisting of two short inspirations to one expiration. At fifteen minutes past two, a.m., the dog died, nine hours and forty-eight minutes after the commencement of the experiment.

Examined twenty hours after death, the following appearances were noted. There was moderate cadaveric rigidity; the brain was slightly congested externally, but presented no bloody points. The lungs were collapsed, dark in patches and congested. The heart was enormously distended, and was remarkable for being excessively loaded with separations of fibrine. The right auricle, pulmonary artery, and the left auricle were literally distended with fibrinous concretions to the almost entire exclusion of the red blood. The right and left ventricles contained a large quantity of dark clotted blood, but there were some separations of fibrine in these cavities; the fibrinous concretions in the right auricle and pulmonary artery were of pure whiteness. Those on the left side were red and striated very closely, resembling muscular fibre. The liver and spleen were congested. The kidneys were normal. The stomach viewed externally had a vascular appearance, but internally the mucous

surface was natural. There was no serous effusion in the alimentary canal, nor any particular inflation with gaseous matter.

Experiment 9. Effects of Sulphuretted Hydrogen.
I placed a dog in the box at 8 a.m., and gradually introduced nine cubic inches* of sulphuretted hydrogen. Within two minutes the respiration became quickened, and he began to stagger. For a quarter of an hour he was restless, and walked with difficulty. His movements were more like those resulting from intoxication than I had ever seen in a lower animal. This effect gradually subsided, and I took him out in three hours. The dog was feeble on removal but recovered perfectly.

Experiment 10. Effects of Sulphuretted Hydrogen.
At eighteen minutes to ten, a.m., a dog was placed in the chamber, and twelve cubic inches† of sulphuretted hydrogen were then introduced. He had violent tremors, and some degree of shortness of breathing within three minutes. At half-past eleven o'clock the breathing was laborious. This continued for some time, when he appeared better, and was removed at half-past three o'clock, not labouring under any morbid symptom.

Experiment 11. Effects of Sulphuretted Hydrogen.
A jackdaw was placed in the box already described,

* Or 0.154 per cent.

† Or 0.206 per cent.

and there were diffused through the air of the box nine cubic inches* of sulphuretted hydrogen. Within two minutes the bird attempted to vomit, and almost instantly afterwards was purged. He was incessantly restless, and the breathing was remarkably hurried and catching. After inhaling the gas for ten minutes, his movements became so feeble that it was with difficulty he stood. The pupils at first contracted, soon became largely dilated. The beaks were set widely open. The tongue, dry and dark at the tip, was protruded at each inspiration. After remaining in this condition for one hour and a half he was removed from the box and soon recovered.

Experiment 12. Effects of Sulphuretted Hydrogen.

A jaekdaw was placed in the box as before, and there were then introduced six cubic inches† of sulphuretted hydrogen. Within two minutes the bird began to vomit (a curious symptom to observe in birds), and he was also freely purged. These symptoms continued for twenty minutes. Afterwards the respiration was very hurried. After keeping him in the box for two hours without much further modification of symptoms, he was removed, upon which he recovered.

Experiment 13. Effects of Sulphuretted Hydrogen.

A dog was introduced into the box. When he was

* Or 0.154 per cent.

† Or 0.103 per cent.

quiet in his new situation, six cubic inches* of sulphuretted hydrogen were very gently driven in. At first there was much watering of the eyes, and this was followed by signs of thirst, muscular debility, and slight drowsiness. In half an hour the breathing had become hurried, and one hour later still he was violently purged, the breathing became more rapid and the tremors more intense. Three hours after his first introduction the respiration was still hurried and the heart's-beat so rapid that it could be counted with no precision. I calculated after several attempts to reckon the beats, that there were at least 240 in the minute. He was now again purged. When the experiment had progressed thus far the animal was removed from the box. He staggered as he walked, but soon recovered in the pure air.

Experiment 14. Effects of Sulphuretted Hydrogen. A common hedge-sparrow was put into the box, as before, with six cubic inches† of sulphuretted hydrogen. Within two minutes he fell down insensible and continued in this state for the space of a minute. The respiration then became very hurried and gasping. He rose, but staggered a good deal, and fell again on his back. Six minutes after the commencement of the experiment he vomited, became convulsed, and died in fifteen minutes.

* Or 0.103 per cent.

† Or 0.103 per cent.

Experiment 15. Effects of Sulphuretted Hydrogen.

A linnet was placed in the same box as was used in the preceding experiment, and without any further introduction of gas. It was put in within ten minutes after the commencement of the preceding experiment. The respiration became hurried at first, but this passed off in the course of half an hour. At noon, i.e., one hour and seven minutes after the introduction into the box, I removed it apparently well, but it died in the evening.

Experiment 16. Effects of Sulphuretted Hydrogen.

A dog was introduced into the box as before, and three cubic inches* of sulphuretted hydrogen were driven in. He suffered almost at once from eructations, and tremors of the muscles. The respiration was also quickened, and the heart beat with extraordinary rapidity. At the same time he seemed sufficiently lively. After keeping him in the box for two hours he was let out. The pulsations of the heart could be heard at a short distance from his body, the action was so intense. After removal he was purged for a few hours, but eventually got quite well.

The three following experiments were conducted by my friend Dr. Richardson, and have been sup-

* Or 0.051 per cent.

plied by him to me, as singularly corroborative of my own observations. They have not been published until now.

Experiment 17. Effects of Sulphuretted Hydrogen.

At 12.30 noon, a dog was placed in a glass chamber containing 3025 cubic inches of air. At 12.45 there was introduced into the chamber six cubic inches of sulphuretted hydrogen gas. Immediately after the introduction of the gas the respirations were counted and found to be eleven per minute; during the first half-hour there was little remarkable except that the respirations were very deep and slightly jerking. At 1.20 p.m. four more cubic inches of the gas were driven in, the introduction causing immediate restlessness, and considerable anxiety in breathing: throughout a little fresh air was often admitted into the chamber by withdrawing one of the feeding stoppers. At 1.25 the animal exhibited symptoms of sickness, the respirations were sixteen per minute and diaphragmatic. At 1.30 the respirations were twenty-four per minute and accompanied with a sharp whistling sound. At 1.50 the respirations averaged forty-five per minute and were double. At 2.15 three cubic inches of the gas were driven in, when the animal fell as if dead, and ceased to respire for a few seconds, but recovered the shock. At 2.30 one more cubic inch of the gas was driven in; the animal appeared

about the same, and the respirations varied from thirty to fifty per minute. At 2.40 three more cubic inches were driven in, and five minutes later an additional three cubic inches, upon which the animal's breathing became stertorous for the first time. At 2.55 one-and-a-half cubic inches of the gas were thrown in, and at 3 p.m. the same quantity again. The animal was still narcotized, the breathing being stertorous. At 3.10 two cubic inches, at 3.15 two cubic inches, and at 3.20 one cubic inch of the gas were driven in. At 3.27 the dog was taken out of the chamber, he was excessively prostrated, and the breathing was deeply stertorous. After a lapse of twenty minutes, the dog was put in the chamber again, and four more cubic inches of the gas were gently diffused in the chamber. At 5.20 four more cubic inches of the gas were driven in, when the dog became very restless for a few seconds, the respirations being hurried and stertorous. At 6 p.m. the animal was languid, with irregular breathing and irregular action of the heart. He was now left for nearly two hours breathing common air, at the end of which time he seemed a little recovered but was quite insensible.

At 8 p.m. twelve inches of the gas were very slowly introduced; at the moment of the introduction of each portion there was a return of the restlessness followed by quick and difficult breathing. There was some insalivation, and a

tolerance of the poison seemed to be established. At 8.15 the respirations were sixteen per minute and deep; six cubic inches more of the gas were introduced. At 8.30 there were general tremors amounting almost to convulsion; great prostration and insensibility; six cubic inches more of the gas were now driven in. At 9.10 the prostration and insensibility still continued, and six cubic inches more of the gas were driven in with a fresh supply of air. At 9.25 the animal appeared the same; six cubic inches more of the gas were again driven in with a fresh supply of air. At 9.40 the prostration still continued, and six cubic inches more of the gas were introduced; the respirations were twelve per minute. At 9.55 the prostration was very marked, and once more six cubic inches of the gas were thrown in with air. At 10.30 the animal seemed more prostrate, the respirations being ten per minute. Four cubic inches more of the gas were thrown in. At 10.40 the animal appeared the same, and two cubic inches more of the gas were introduced with air. At 11 p.m. the animal appeared the same; two more cubic inches of the gas were driven in. At 11.30 the animal appeared more prostrate, and was entirely insensible. Two cubic inches of the gas were driven in. At 11.45 the animal seemed still more prostrate; two cubic inches of the gas were slowly injected into the chamber. At 12.20 two more cubic inches of the gas were thrown in. At 12.45, the coma being

profound, the animal was taken out of the box and exposed to the pure air, which he was allowed to breathe until two. At this time the body was intensely cold, and the action of the heart almost imperceptible; at the moment when breathing had stopped tracheotomy was performed and artificial respiration kept up with a pair of double-acting bellows. The chest was now laid rapidly open, when the heart was found feebly pulsating; the blood on both sides of the heart was fluid, but in the pulmonary artery there was a small separate fibrinous clot. The lungs were pale, and the structure of the kidneys, liver, and the surface of the alimentary tract were also pale. The blood, on exposure, coagulated readily. The brain and spinal cord were healthy.

Experiment 18. Effects of Sulphuretted Hydrogen.

On the 27th December 1859, a dog was placed in a chamber containing 3033 cubic inches of atmospheric air. At 12.10 p.m. six cubic inches of sulphuretted hydrogen gas were gently thrown into the box. Immediately after its introduction the dog ceased to respire, and it was found necessary to allow him to breathe the ordinary atmospheric air, in order to re-establish the respiration; as soon as this was accomplished he was placed in the box again. At 12.30 p.m. there was considerable jactitation of the muscles; the respirations were

sixteen in the minute, and the dog was nearly insensible. Two cubic inches of the gas were now thrown into the box. At 12.45 p.m. two additional cubic inches were thrown in, upon which the dog became very restless, and the difficulty of breathing more marked.

At 1 p.m. the dog was found breathing quietly. On two cubic inches of the gas being introduced he began to breathe rapidly for a minute, then the breathing ceased for more than half-a-minute, when it was re-commenced with great rapidity, the respirations being forty-four per minute, and double; there was also insalivation. Defecation and micturition took place at this time. At 1.15 p.m. two cubic inches of the gas were introduced: the respirations were now eighteen per minute; the breathing diaphragmatic and stertorous. At 1.30 two cubic inches were introduced, and as soon as the irritating effects of the gas had subsided the animal appeared to be profoundly narcotized. The respirations were fourteen per minute, and double, the expiratory effort being preceded by a peculiar jerking of the abdominal walls, the act being also accompanied with a loud noise. The mouth was slightly opened and the angles drawn back during each inspiration. At 2 p.m. the dog being now in a comatose state, half-an-hour had been allowed to elapse before fresh gas was introduced; two more cubic inches were now gently diffused through the chamber, the time occupied by the introduction being four minutes.

The respirations were thirty-six per minute. At 2.20 common air was admitted, when the respirations were thirty-two per minute and hurried; the dog was still insensible. At 2.30 two cubic inches of the gas were again introduced; the gas was still very irritating, and the expirations very much prolonged. The dog ceased to whine when fresh gas was introduced. At 3 p.m. two cubic inches more of the gas were introduced; the dog was still narcotized; the time occupied in the introduction of the gas was about ten minutes; the irritability which the animal previously manifested had now entirely disappeared. The dog reined on his left side, with the legs extended, (he had been in this position for the last hour, and was still insensible). At 3.30 two cubic inches of the gas were again introduced; the narcotism still being present. At 4 p.m. two cubic inches of the gas were introduced; the dog appeared about the same. At 4.20 the dog was taken out of the box and allowed to breathe common air. Both sounds of the heart were audible. The extremities and surface of the body generally were found cold. At 8 p.m. there were slight gasping respirations, and the animal seemed to be dead, although he had continued to breathe pure air. The stethoscope applied failed to detect the heart's sounds. Tracheotomy was instantly performed and artificial respiration set up by means of a pair of double-acting bellows. The chest was now laid open, and the heart (when liberated

from the pericardium,) was found to be in action, the auricles pulsating, and the ventricles slightly contracting. The heart was very large; especially its right side. Both cavæ were full of blood. The right side of the heart was now laid open, the incision extending through ventricle and auricle, these cavities were full of thick black blood, but no perceptible clot was found. The blood was readily coagulable. The left side of the heart laid open, revealed a little blood in the ventricle but no clot. The aorta and pulmonary arteries were next examined, and a small clot of blood was found in each, but it appeared to be a post-mortem effect. The lungs were remarkably white, and they, as well as the heart and the parietes of the chest, were very cold.

Experiment 19. Effects of Sulphuretted Hydrogen.

On December 29th 1859, a bitch was placed in the same box used for the two preceding experiments. At 10.40 p.m. four inches of sulphuretted hydrogen gas were introduced; this had little effect beyond making her "sniff." At 11.5 p.m. four more cubic inches of the gas were thrown in. She had now severe difficulty in breathing. At 11.15 the respirations were fifteen per minute, each respiratory act being accompanied with slight trembling. The animal continued quite sensible, and as yet the gas had not caused much irritation. At 11.30 fresh air was admitted, and then an additional four cubic

inches of the gas. The animal now became very restless, the respirations were sixteen per minute, the expiratory act being prolonged and accomplished with great difficulty. At 11.45 the respirations were diaphragmatic, numbering twenty per minute. The animal appeared to be in a state of narcotism. At 11.50 the respirations were forty-three per minute, the breathing double and jerking, loud and stertorous. The bowels had been moved and micturition had taken place. At 11.55 the respirations were fifty-four per minute. At 12 p.m. the animal appeared to have recovered somewhat. Two more cubic inches of the gas having been introduced, and the animal became restless for a few seconds. At 12.15 a.m., December 30th, the supply of air was renewed, and two cubic inches of the gas again thrown in. The animal was now quite insensible, the eyes having a dull appearance, the jactitation was also more marked. At 12.35 two more cubic inches of the gas were introduced, the jactitations still continuing, and the respirations being eight per minute. At 12.45, the animal being perfectly narcotized, she was taken out of the box in order to examine the heart; the sounds were found to be just distinguishable, but the rhythm was very imperfect; during inspiration the heart beat rapidly for eight or nine times, and then ceased for a time as soon as the expiratory effort commenced, and during the time occupied for the act, which was much longer than the inspiratory act, it was

observed to beat three or four times very slowly. The animal was now re-introduced into the box, and at 12.50 two cubic inches of the gas were thrown in. At 1 a.m. the respirations were ten per minute, the breathing diaphragmatic and stertorous; the teeth and tongue were covered with a black fur, and there was also some insalivation. At 1.15 the finger applied to the femoral artery discovered the pulse to be eighty per minute, small, compressible, and very irregular; at first quick, then at rest, and lastly slow; these three forms of change recurred with marked character. The respirations were fourteen per minute. Slight twitchings of the limbs and nodding of the head took place occasionally. At 1.30 a.m. she was taken out of the box, and on examining the chest loud vesicular breathing was found to be everywhere present. The heart's sounds were still audible though very dull. The jactitation was now extremely well marked. At 1.45 a.m. she was left for the night, in pure air. At 9.30 a.m. the animal was still alive; the face and extremities very cold, the eyelids drooping; there was well marked jactitation, and perfect insensibility. Tracheotomy was now performed, and respiration kept up by the aid of a pair of double-acting bellows. The chest-wall was removed and the pericardium opened. The auricles and ventricles were still contracting, the ventricles were under the influence of spasm, a wavy action passing from the apex to the base. The right

side of the heart was now laid open ; it was found quite full of blood which soon coagulated. The left side was next laid open, and no blood was found in this cavity. The aorta and pulmonary artery were next examined ; there was some coagulated blood in the latter vessel, but no fibrinous concretion. After all the heart's cavities had been examined, a very slight stimulus was quite sufficient to make the muscle contract. The lungs were quite bloodless, and of a pinkish white colour ; the liver and spleen were healthy ; the kidneys healthy but very white. The urinary bladder was distended with fluid. The stomach and intestines were very white, the inner surface of the stomach being slightly injected. The brain and spinal cord were healthy but quite bloodless.

3. EXPERIMENTS WITH SULPHIDE OF AMMONIUM.

Experiment 20. Effects of Sulphide of Ammonium.

A large dog was placed in the box as before, and six drachms of sulphide of ammonium were introduced. He soon suffered from lachrymation, salivation, restlessness, and vomiting. The vomited matters gave off white fumes. There was a peculiar noise during expiration. In five hours he had recovered, and was then removed.

Experiment 21. Effects of Sulphide of Ammonium.

Another dog was put into the box, and five drachms of sulphide of ammonium were diffused through

the box. He presented the usual symptoms of salivation, lachrymation, and general distress. He was removed in seven hours, having recovered.

Experiment 22. Effects of Sulphide of Ammonium.

A dog was placed in the box with half an ounce of sulphide of ammonium. For ten minutes he laboured under violent excitement, with lachrymation and salivation. He also had some tremor and tenesmus. The symptoms subsided, and he was removed from the box in five hours. I noticed that the numerous fleas with which the dog was infested came to the surface during the experiment and died.

Experiment 23. Effects of Sulphide of Ammonium.

A jackdaw was placed in the box and half an ounce of sulphide of ammonium introduced. The breath was distinctly observed to produce white fumes, as also did the fæces. The membrana nictitans was very active. The bird vomited, and the vomited matters were of a yellow colour; the beaks were separated; the tongue was dry and dark coloured at the tip. He was much purged, and the fæces were liquid. He expanded both his wings to support his body. The respiration became quicker, and he died in two hours.

After death the blood was fluid; the lungs were congested; the brain was congested; the other viscera were healthy.

Experiment 24. Effects of Sulphide of Ammonium.

A dog was placed in the box and three drachms of sulphide of ammonium were introduced. Lachrymation and salivation were profuse. He also had some degree of tremor. These symptoms gradually subsided, and the animal was removed in four hours and a half, having slept for a short interval in the meantime.

Experiment 25. Effects of Sulphide of Ammonium.

A jackdaw was placed in the box with three drachms of sulphide of ammonium. He quickly made attempts to vomit, and was purged. The breathing became hurried, and the breath produced distinct white fumes. In two hours from the commencement of the experiment, the breathing was very laborious and very noisy. The bird appeared then to be very feeble, and his gait was very tottering. He was left in the chamber with the impression that he would die during the night, but we found him alive at nine o'clock the next morning. He had been purged during the night; he appeared to have recovered, and did recover.

Experiment 26. Effects of Sulphide of Ammonium.

A dog was placed in the box with one ounce of sulphide of ammonium. He soon laboured under profuse lachrymation and salivation, and became very restless. Within five minutes tenesmus showed itself.

The respiration became hurried and difficult. He died in ten minutes. The bowels were evacuated after death. At the post-mortem examination made twenty-four hours afterwards, the right auricle and ventricle were found filled with liquid blood. The left cavities contained a small quantity of fluid blood. The venæ cavæ were distended with fluid blood. Both lungs were deeply congested, and of a dark colour. The vessels of the brain were congested. The stomach was distended with food and an offensive gas. It presented a reddened appearance of the mucous surface. The liver, kidneys, spleen, and intestines were of healthy appearance.

Experiment 27. Effects of Sulphide of Ammonium.

A dog was put into the box and two drachms of sulphide of ammonium were introduced. Occasionally in the course of the experiment a current of air was drawn through the chamber, so as to prevent the accumulation of carbonic acid gas. The vapour at first produced irritation of the eyes which soon passed away. While in the box the animal refused food and drink. After inhaling the vapour for twenty-four hours, during which time the dose was repeated, he was removed from the box. He was exhausted and refused food. He rested for a while, then ate, and began to run about more actively. There was nothing peculiar about the heart or

respiration. A drop of blood was drawn from the ear and examined microscopically. There was some slight irregularity of corpuscle, but nothing very marked. The animal was returned to the box at 1 a.m., and the administration of the vapour was re-commenced in two draehm doses. At 6.30 a.m. we found the dog lying upon his side, the surface was cold, the tongue protruding and cold, the pupils dilated, the respiration feeble, the breath very ammoniaeal, and the pulse feeble and irregular. There was jactitation of the fore legs, and occasional drawing up of the hind legs. The dog was now left in the open air, but the breathing became somewhat more feeble. The jactitation of the fore paws increased, as also did the drawing up of the posterior extremities; he remained comatose and insensible, and died at 8 a.m.

The post-mortem examination was made thirty-six hours after death. The limbs were very rigid; the right lung was vermilion-coloured; the lower and middle lobes of the left lung were congested (hypostatic). The heart, both on right and left side, was filled with feebly coagulated blood, of a very dark colour. The mucous membrane of the stomach was injected and softened. The liver was healthy. The gall-bladder was distended. The intestinal mucous surface was somewhat injected. The kidneys and spleen were healthy. The surface of the brain was injected with dark venous blood.

Experiment 28. Effects of Sulphide of Ammonium.

At 9 a.m. a jackdaw was put into the box and two drachms of sulphide of ammonium were introduced in a saucer. As the vapour became diffused he shook his head violently and continuously, and the membrana nictitans was brought rapidly into action. The breathing also became quickened, and in a quarter of an hour he stretched out his neck and opened his beaks frequently in a gasping manner and vomited. He was purged, and the fæces gave off white fumes. The breath also formed distinct white fumes as it was evolved. The tongue became very dry and dark coloured at the tip. The quick breathing and frequent opening of the beaks continued about two hours. He then fed himself, took water greedily as if thirsty, but did not vomit more. After three hours I removed him from the box, when he quickly recovered.

Experiment 29. Effects of Sulphide of Ammonium.

A hedge-hog was placed in the box with two drachms of sulphide of ammonium. He breathed with his mouth open, and appeared to be uneasy. Lachrymation was produced, and white frothy mucus flowed from the nostrils. He afterwards became drowsy. Having recovered he was removed in four hours.

Experiment 30. Effects of Sulphide of Ammonium.

A dog was introduced into the box as in previous experiments. Before introducing him I ascertained

the pulse-beat to be at the rate of 160 per minute. When he was composed there were put into the box, in a saucer, two fluid drachms of the sulphide of ammonium. As the gas diffused through the box, the animal suffered from severe lachrymation and salivation. When these signs passed away he became restless, and trembled. The respirations were hurried, catching, and irregular. After a time the breathing became heavier, the expirations being forced and prolonged, and the abdominal muscles acting violently and spasmodically. There was however no purging nor vomiting. The heart-beats became extraordinarily quick, at times numbering 220 per minute, and being so violent that the action could be counted from the chest-wall. He ultimately recovered.

4. EXPERIMENTS WITH CARBONIC ACID.

Experiment 31. Effects of Carbonic Acid Gas. A hedge-hog was placed in the box, and eighty-eight cubic inches of carbonic acid gas (1.51 per cent.) were introduced. For a quarter of an hour he remained curled up, he then breathed more quickly, sometimes irregularly, and occasionally drew a long inspiration. Soon afterwards he was very restless, running about and trying to escape, he was also purged. He became quieter afterwards, apparently from exhaustion. The animal was removed in four hours and a half, and recovered.

Experiment 32. Effects of Carbonic Acid Gas. A dog was placed in the box, and one hundred and sixteen cubic inches (2 per cent.) of carbonic acid gas were slowly introduced. With the exception of slight restlessness at first, and very slightly embarrassed respiration, no further effect was produced for two hours. He was now left with the chamber open freely to the air, and I found him dead in six hours and a half from the commencement of the experiment. The limbs were quite flaccid, and urine passed freely on removing him from the box. On post-mortem examination thirty hours afterwards, the right auricle and ventricle were found moderately distended with dark-coloured blood partly coagulated; the left cavities of the heart were but slightly distended with coagulated dark-coloured blood. No fibrinous deposit existed in any of the cavities. The venæ cavæ were distended with partially coagulated blood. The lungs were of a bright vermilion colour. The vessels of the brain were congested with dark-coloured blood, and the cut cerebral surfaces dotted with bloody points. The stomach was full of food, and its mucous surface reddened. The liver, kidneys, and intestines were healthy.

Experiment 33. Effects of Carbonic Acid Gas. A dog was put into the box as before, and eighty-eight cubic inches (1.51 per cent.) of carbonic acid gas were slowly introduced. In half an hour

the breathing became somewhat quicker and oppressed. In two hours and a half the breathing was still quick and laborious, and the animal was more feeble. I then admitted some fresh air; an effort to vomit was made. I removed him in four hours and a half, at the end of which he appeared to have recovered.

Experiment 34. Effects of Carbonic Acid Gas. A dog was placed in the box and 290 cubic inches (five per cent.) of carbonic acid gas were introduced. In half an hour the respiration became somewhat laborious and quickened. In an hour the respiration was considerably more hurried—sixty per minute. In an hour and a half the respiration was sixty-eight per minute, and saliva was flowing from the mouth. The upper opening of the box was now unclosed so as to admit air, and the animal was left for eleven hours. During this time there were no signs of coma or insensibility. The breathing continued quickened, laborious, and wheezing. There was also great debility, but neither vomiting nor purging. I removed him from the box at the end of twelve hours and a half. He was very weak; the respirations were fifty-two per minute and noisy. The respiratory murmur was loud, coarse, and dry. The heart-beat was quick and feeble. On removal recovery took place.

CHAPTER XIII.

INFERENCES AND SUGGESTIONS FROM THE PRECEDING
EXPERIMENTAL INQUIRIES.

Poisonous Effects of Cesspool Emanations. Effects of Sulphuretted Hydrogen:—analogy with Typhus. Effects of Sulphide of Ammonium:—analogy with Typhoid. Effects of Carbonic Acid Gas.

The experiments related in the preceding chapter were given without comment, and inasmuch as they offer to the intelligent reader an efficient history in themselves, it is not necessary for me to dwell at any considerable length in the way of argument or illustration.

In the first place it cannot now be doubted that cesspool emanations when steadily inhaled, become poisonous. The dogs subjected to the cesspool air were all affected more or less. The symptoms were those of intestinal derangement, followed by prostration, heat of the surface of the body, distaste for food, and those general signs which mark the milder forms of continued fever common to the dirty and ill-ventilated homes of the lower classes of the community. At the same time it is clear that for the production of the symptoms described two

conditions were necessary. One, that the animals should be confined in a close atmosphere containing the gas, and two, that the confinement should be continued over a considerable interval of time. This latter fact accounts for the immunity of medical men from the cesspool poisons, when they are performing their ordinary professional duties. Passing into the infected room and remaining there but a brief space of time, the medical man, on returning to the fresh air, throws off the emanation before it has time to take effect, while friends and nurses who are kept constantly around the sufferer, breathing the same air, become affected.

The experiments illustrate further how short a distance of removal from cesspool air suffices to render it innocuous, if anything like an opportunity is allowed for gaseous diffusion. For example, the same animals which suffered in the chamber containing air coming direct from the cesspool, were quite unaffected by it in a kennel not ten yards off. It will be observed also, as one of the symptoms produced by the foul emanations from the cesspool, that the appetite of the animals was entirely suspended: with this suspension, lasting over many days, there was, nevertheless, much more marked evidence of remaining strength than could have been expected as the result of deprivation of food in the open air. I infer, necessarily, that in truth the waste of the bodies of the animals was less active,

that the blood was not undergoing active oxidation, and that the food was not required. I have made the same inference in regard to persons suffering from typhus: in them there is little desire for sustenance because there is little demand.

The peculiar poisonous action of sulphuretted hydrogen is well illustrated in these experiments. It will be observed that the symptoms produced even by the same dose differed in degree in different animals of the same class, the one animal dying from the effects of a dose which was insufficient to do more in the other than produce dangerous symptoms. The symptoms arising from sulphuretted hydrogen are well marked, and may be considered specific. Vomiting and purging are the most prominent symptoms. The purging is painful and straining, the vomiting is difficult and exhausting, and eventually there is insensibility and entire prostration. When the dose of the poison is at first very large, the prostration and the insensibility are immediate. The pathology following such poisoning is definite. If the death takes place quickly, the pathological evidence is the evidence of asphyxia; if the poison is long breathed in diluted doses the pathology is modified; the lungs, liver, kidneys, and mucous surfaces are blanched, and death occurs, as from coma and anæmia. It is worthy of remark that the blood in these examples loses none of its coagulating power; nay, that

occasionally when life is very prolonged, the fibrine may undergo separation and clog up the cavities of the heart. The dose of sulphuretted hydrogen required for the production of the specific symptoms is tolerably well shown. It is clear that so little as 0.429 per cent. is a dose absolutely and rapidly poisonous, that so little as 0.206 per cent. may be fatal, and lastly, that so minute a dose as 0.051 per cent. is sufficient to produce serious symptoms, eructations, tremors, rapid and irregular respiration, extraordinary rapidity of the pulse, and diarrhœa. One other point respecting the effects of sulphuretted hydrogen is also worthy of special regard. It is this: that after a certain degree of exposure to the gas, removal from its influence and restoration to the open and pure air do not lead to restoration of life. The coma continues, the exhaustion continues, and death results. Hence sulphuretted hydrogen, different to other volatile gases, fixes an impression on the blood which is more or less permanent, resembling in this respect the poison of typhus.

The effects of sulphide of ammonium, while they differ from those produced by sulphuretted hydrogen, are in themselves sufficiently distinct. Vomiting is a symptom of this poison, without purging, but occasionally with tenesmus. When the dose is very large death occurs speedily, with quickened and laboured respiration. When the administration is

kept up in small doses for many hours, the symptoms are those of excited circulation and thirst, followed by rapid sinking. The surface of the body from being unusually hot, becomes unusually cold, the tongue is protruded, dry, dark, and cold. There is constant jactitation of the limbs, subsultus tendinum, feeble quick pulse, and ultimately death, which may occur even some hours after the animal is removed from the poison and is placed in the open air. There is only one word of a familiar kind which expresses the symptoms arising from sulphide of ammonium, and that is "typhoid." The symptoms are essentially and purely typhoid in character.

The pathology after death from sulphide of ammonium differs from that which follows the administration of sulphuretted hydrogen. When the inhalation is prolonged and the death is gradual, the alimentary mucous surface is changed. The mucous coat is injected and softened in patches. The blood shows no fibrinous separations, but is dark, and either feebly coagulated or entirely fluid. The blood corpuscles are also much dissolved and changed, and there is congestion of fluid blood in all the vascular organs.

The dose of sulphide of ammonium required for the production of serious symptoms is difficult to calculate, and this from the fact that when the vapour of the sulphide is diffused through a confined space in which an animal is breathing, there is

quickly a deposit on the floor of the chamber of the white bicarbonate of ammonia. This deposition is so rapid indeed, that the effect of the poison is very quickly lost, so that constant renewal is required, and the calculation of dose is necessarily rendered obscure, since the animal is not breathing the same dose for any two minutes together.

The symptoms arising from carbonic acid gas have been described so often by various authors that I need not dwell on them, nor have I pressed them far experimentally. The respiration suffers first, from this poison; there is prostration, and if the inhalation is prolonged, purging. The effects vary with the dose. The instances I have given above are the effects of a small long continued dose. In larger proportions insensibility, and coma, and asphyxia are the results. The pathology varies. While congestion of the lungs is commonly noted as the leading pathological sign, it is clear from one of my experiments, that when the gas has been long inhaled in small quantities, this is not necessarily a pathological result, for in one of my cases the lungs were found of a bright vermilion colour and free from congestion. The effect of carbonic acid gas on the blood is definite; it neither produces fibrine deposit nor complete fluidity; but there is feeble coagulation and a dark colour even in the arterial blood. Breathed continually for a long time, in a very minute dose, the brain suffers seriously by

becoming congested with blood, and the mucous membrane of the stomach is injected and reddened. When the gas has been breathed for a long time in small quantities, so as not to produce insensibility, the effect does not pass off so rapidly on placing the animal in the open air, as is generally believed. In the second experiment with carbonic acid, narrated at page 207, the animal after being exposed to an atmosphere for two hours, in which he breathed from the first two per cent. of carbonic acid, was left (not apparently suffering much) with pure air entering freely into his chamber, yet he died after all. The smallest dose of carbonic acid required to produce dangerous symptoms, cannot be determined absolutely from the experiment of placing an animal in a closed chamber, and introducing the gas, inasmuch as the gas is also streaming off from the animal itself. I think however the inference is quite fair, that from one to two per cent. of this gas is sufficient, when long inhaled, to produce decided symptoms of imperfect oxidation of the blood, and all the after prostration incident to such interference with the primary act and principle of life.

The symptoms which we have thus noticed as resulting from the inhalation of sulphuretted hydrogen, sulphide of ammonium, and carbonic acid, are sufficient to account for the effects arising from cesspool emanations, without seeking for any further product from such emanations. Comparing the

experiments with cesspool air, with those in which separate gases were employed, the inference seems clear to my mind, that the symptoms arising from the inhalation of the cesspool atmosphere were due mainly to the presenee of a small amount of sulphuretted hydrogen, which gas with ammonia was always present. If the experiments with the cesspool air be placed side by side with those in which sulphuretted hydrogen, in the proportion of 0.51 per cent. was administered by inhalation, the analogy between the two sets of results will be sufficiently unmistakeable.

CHAPTER XIV.

ON PUBLIC AND PRIVATE HYGIENE IN RELATION TO THE
DISEASES UNDER CONSIDERATION.

A Sanitary Code. Ozone as an Atmospheric Disinfectant. Dr. Angus Smith's Sepometer. The Detection of Organic Impurities in Water.

So much is said in these days on sanitary improvements, that it is impossible to write any remark which may not be found already written. Such views as have been expressed in this book, with regard to the diseases under discussion, do not modify the principles of sanitarians, but strengthen them and show their full importance.

Within these last few years we have seen an actuary, Mr. Neison, laughing at all sanitary measures, and assuring the world that such measures do not lessen mortality; and that cleanliness as next to godliness is an ignorant axiom. I would write a few words therefore at the risk of repetition, if it were only to refute Mr. Neison's figures and deductions.

That comprehensive sanitary regulations, in other and more simple terms, regulations for general cleanliness arrest disease and lessen mortality,

admits only of particular proof from particular cases, and of general proof from logical arguments founded on a correct general knowledge of the origin and course of epidemics. It is unfortunate that the mere statist is too often not aware of these facts, and is therefore misled himself and made to mislead others. He forms a general conclusion from a general calculation, which is right in the abstract, but which it is wrong to use in condemnation of measures which have never yet been universally applied, and cannot consequently be discussed on universal data.

In considering the influence of sanitary measures we must consider them in their special application, we must not take all England, nor all the world, and judge on such grand bases of calculation; but we must be content to take special localities where the sanitarian has tried his skill, and by comparing the health and longevity of these localities before and after the sanitarian entered them, we must draw our conclusions from the particular observation. In the chapter of cases (chapter x.) I have given so many illustrations, showing the beneficial influence of sound sanitary provisions in reducing mortality, and removing epidemics from villages and towns, that it were useless here to enter further into this controversy. The fact is self obvious, the statist's deduction from general data will show a false deduction against sanitary improvement, until such

time as sanitary laws have a general instead of a special and limited application.

In the extension of hygienic principles for the prevention of the diseases we have been considering, it is always important and necessary not to take half measures, but to carry out the cleansing process to its fullest extent. It is requisite to take no one-sided view as to causes, but in our present uncertainties to remove all possible causes, whether terrestrial, aqueous, or aërial. To remove a manure heap or ash-pit and leave a foul well is one of the foolish things which has been done, but which will surely never be repeated.

Were a sanitary code to be established for this or any other country, its provisions to be sound would be as simple as few.

The first provision in such a code would secure—that brick and cement should form the wall of every well from which water is procured.

The second—that brick and cement should seal over every cesspool and sewer.

The third—that perfect drainage-flow and perfect flushing of drains should be secured to every town and village.

The fourth—that sufficient house-room and plenty of pure air be supplied to every man.

The fifth—that whenever an epidemic should show itself, the cases should be isolated and the excreta of the patients be immediately removed.

This is the simple sanitary code required ; for as the causes of diseases have human bodies for their habitat and starting points, and as one person may propagate a disease to a thousand as easily as to one, so the prevention of disease in one individual may save the thousand from the ravages of the disease as well as the one, and so mortality will be lessened wholesale, as diseases are stopped individually.

Private hygiene, or as it has been called “hygienic treatment,” such as the physician has to exercise, is included in securing obedience to the general laws. Give the man sick with epidemic disease fresh air,—give him uncontaminated food and drink,—remove his excreta carefully, and isolate him as far as possible. For the disease is in the man, and if it be sharply policed it will travel no further.

These rules are not mere statements, they are the results of experience. I have traced the most contagious of our epidemics, scarlet-fever, into large schools, and have confined its ravages to the imported case, by isolating the patient. I have seen a whole population saved from the recurring havoc of typhoid by a new provision for so disposing of the excreta, that men shall not have such excreta again till the vegetable world has disinfected them, and every week of my life adds some new fact to the truth of this argument.

Whether the specific poisons of the specific diseases will ever be isolated, analysed, and described, is an open question. We are far from such a point now, and if preventive medicine progresses, the diseases we have treated of may, perhaps, happily die out, before science has attained to the discovery of their first causes.

In concluding this chapter relating to hygiene, I may briefly remark that for some years I have regarded ozone as nature's great atmospheric disinfectant. I have therefore prepared it artificially, in the way that Schönbein first proposed, and used it advantageously in situations where free ventilation of pure air could not be secured. I may, perhaps, at some future time give the results to the profession.

We are indebted to Dr. Angus Smith for having introduced a beautiful process for the examination of the purity of the air, by means of the alkaline permanganates, and an apparatus which has been termed the Sepometer. He gave an interesting series of observations on the relative quantity of oxidizable matter present in different specimens of atmospheric air, first at the Chemical Society, in 1858, and afterwards at the Royal Institution in 1859.* Dr. Smith found that the air of Manchester required 52·9 measures of permanganate solution to decompose the impurities contained in 100 cubic

* Quarterly Journal of the Chemical Society.—Athenæum, June, 1858.
Chemical Gazette, vol. xvii.

inches, while that of the country near Manchester required only 13·7. The air of the German Ocean, 60 miles from Yarmouth, required only 3·3 measures to take up the oxidizable matter which it contained, whereas the air of an uncovered pigstye required 109·7 measures to neutralize it. A simple "air-test" capable of indicating the relative purity of the atmosphere, without the necessity of delicate manipulation, is still a great desideratum. Condyl's "Tell-tale air-test" is a simple application of the permanganates to the same purpose, and will give valuable approximate results, as I have repeatedly verified by experiment. It cannot, however, be relied upon for philosophical precision.

The readiest method of detecting organic matters in water, and of demonstrating the relative purity of different specimens, appears to be that which was first proposed by Mr. Condyl.* The permanganates are not only adapted to detect organic impurities, even when the microscope may fail to demonstrate them, but, by virtue of the property they possess of rapidly decomposing organic matters, they are a most efficient means of purifying contaminated waters.

* Air and Water: their Impurities and Purification. By H. B. Condyl, 1862.

CHAPTER XV.

SUMMARY OF FACTS AND CONCLUSIONS.

In the succeeding brief chapter I include in a few paragraphs the results to which I have been led regarding malaria; for the sake of distinction I shall put down these results in order, one by one, each as a definite theorem.

I.

I define as a malarious agent, any agent whether organic or inorganic, which being so diffused through the air, as to admit of being inhaled, possesses the power of exciting specific symptoms in the person who inhales it.

II.

The diseases placed before me for consideration, viz., typhus and typhoid fevers, cholera, and the exanthemata, have each their origin in a specific poison which is organic.

III.

The fact of the organic nature of the poisons is proved by their power of reproduction, their

communicability from person to person, their destruction at a temperature extremely high, and the suspension of their activity as poisons at a temperature extremely low.

IV.

Meteorological changes alone are quite insufficient to account for the origin and spread of the above named diseases. But meteorological peculiarities may affect the course of those diseases by exerting an influence on the organic poison. A low temperature may arrest the poison for a time; a warm temperature with moisture may, and in all probability does favour the reproduction of those poisons whatever may be their nidus, the bodies of men, or the earth, or water. Again, when the thermometer is high and the atmospheric pressure is diminished the diffusion of malarious products is increased. Thus on the eve of rain we all know that disagreeable emanations from cesspools are most distinguishable. The vapours producing the bad odour are at such times rapidly evolved, and with them organic matters which may or may not be poisonous, but which being poisonous are then readily diffused.*

* At the same time sudden changes in the temperature and pressure of the air may for the moment produce an effect on human bodies; for instance, a sudden and marked fall in the temperature may produce amongst people unprovided for the change, a general attack of diarrhœa. But this class of disorder does not assume a continued epidemic character, never becomes communicable, and is not usually fatal in its issues.

V.

The organic poisons which excite the diseases under our consideration obey the same laws as regards generation and propagation as do other and more complicated representatives of the organic series. Under favouring conditions to each successive act, they arise, reproduce, and die.

VI.

These organic poisons, infinitely minute in themselves, reach the bodies of men through two sources,—through the air taken in by the lungs,—through the matters taken in by the mouth. With water at ordinary temperatures as their medium, they are undestroyed, in the air they are undestroyed, in any medium they are undestroyed if the temperature and chemical character of such medium is compatible with the persistence of organic life.

VII.

Some of the organic poisons, the poison of cholera for example, seem mainly communicated to man by the alimentary surface, and travel but small distances by the air. They are, however, transmissible through either of the mucous surfaces, the position being only allowed, that they *reach* the mucous surface and alight upon it. The poison of typhoid seems communicable both through air and water: the poisons of the exanthemata seem mainly to be transmitted by the air.

VIII.

In filthy localities, in cesspools, in sewers, in decomposing organic remains, other poisons inorganic in character are generated. These poisons are capable of producing certain specific symptoms analogous in many points to symptoms caused by the organic poisons. Thus sulphuretted hydrogen produces intestinal disorder and prostration; while sulphide of ammonium sets up a class of symptoms resembling closely those of typhoid fever.

IX.

These inorganic poisons are not competent for the production of communicable disease, the symptoms they produce being confined to the body in which they (the symptoms) are demonstrated.

X.

The connection which exists between the organic and the inorganic poisons is the connection of coincidence. Both find the same localities the most favourable points for development, and the two often go side by side.

XI.

Thus, while the specific actions of these two classes of poisons must not be confounded together, their possible connection must not be overlooked. The presence of the inorganic poison in the person of the sufferer, may materially intensify the action of

the organic poison, and even dictate the type of the disorder. As 0·051 per cent. of sulphuretted hydrogen diffused through the air, a proportion imperceptible to the senses, will affect a healthy animal, sicken it, and prostrate it; it is a self evident proposition that the same agent present in the air, and breathed by the fever-stricken person, will intensify his symptoms and influence their course.

XII.

While it is impossible that an inorganic poison can ever be the prime source or generator of organic poisons producing specific disease, it is certain that in the course of certain systemic diseases (such as peritonitis) which are not considered communicable, organic products may be formed, which products, conveyed to susceptible persons, may communicate at least the disease puerperal fever, which in its turn is communicable.

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METEOROLOGICAL FIGURES.

In page 95, reference was made to the meteorological elements upon which the tables in that chapter had been based, and at the time I made that reference it was my intention to append the figures. Further reflection, however, has induced me to decide not to give this dense and unwieldy mass of figures. It would seem to be unnecessary to give them in this place, inasmuch as they have all been published in the *Association Medical Journal* for 1853, 1854, and 1855. To that Work, therefore, I would refer any of my readers who may be interested in these details.

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ERRATA.

Page 107, line 1, *for* "causes" *read* "cases."

Page 129, line 10, *for* "1851" *read* "1852," *and for* "1852" *read* "1851."

Page 131, line 22, *leave out* "proving fatal in some cases."

Page 138, line 8, *for* "tympanitis" *read* "tympanites."

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“ * * * In another column some account will be found of the remarkable observations of Dr. Barker, of Bedford, bearing out these recommendations of Dr. Lankester.”—*Medical Times and Gazette*.

“On the other hand, Dr. Barker gives us experimental proof of the noxious influence of sewer emanations, and of the individual gases which prevail in the cloacæ, and make their way to the lungs and noses of Her Majesty's lieges through the untrapped gully-holes of the streets and open mouths of conduits discharging into the Thames. It is interesting to observe the close resemblance of the symptoms produced in the animals experimented upon to the typhoid fever of man, especially when subjected to the influence of sulphide of ammonium.”—*British and Foreign Medico-Chirurgical Review*.

ILLUSTRATIONS OF THE ORIGIN AND PROPAGATION OF CERTAIN EPIDEMIC DISEASES. A paper read before the Epidemiological Society of London; and reprinted from the Transactions of the Society. 8vo. One Shilling.

London: John W. Davies, 54, Prince's Street, Leicester Square.

